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ORIGINAL LECTURES.

ON PHAGOCYTES.

An Address

before the Alumni Association of Bellevue Hospital, New York,
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(Concluded from page 396.)

THE theory elaborated by Metschnikoff had been hinted at by many previous observers, but to him is undoubtedly due the credit of bringing it into prominence, and of doing in connection with it a very large amount of interesting work. It must be allowed that he came to his task well prepared. Many of us can look back with pleasure to his brilliant investigations upon the intracellular digestion in the Planariæ and in Sponges, carried on largely at the Naples Marine Station; investigations the truth of which, so far as I know, has not been controverted. Following these studies, directly in the same line, was his interesting research into the method of the absorption of the tail of the tadpole, already referred to, in which he appears to have demonstrated that the atrophy of this organ results in reality from the active removal of the fragments of the tissue by leucocytes.

So far the work was biological, and had no direct bearing upon the phenomena of disease further than that, in the latter illustration, it bore out the well-known fact of the absorption by leucocytes of foreign bodies placed within the tissues. In 1884, in the 96th vol. of Virchow's *Archiv*, he published a paper¹ which arrested the immediate attention of students in parasitology. It is now too old a story to narrate at length; it will be sufficient to remark that in the daphnia, the common water-flea of the aquarium, he had studied the relation of the leucocytes to a fungus with which these insects are prone to be infected. The phagocytes attack the fungi which enter the body cavity from the intestines, and practically eat them, enclosing them in protoplasm. Where one cell is insufficient, several combine to enclose the spores in large plasmodia-like aggregations—giant-cells. If the invasion was in such large numbers, and the activity of the fungus so great that conidia were formed, the resisting forces were insufficient, victory remained with the enemy, which meant the death of the daphnia. Of 100 insects studied, 73 became infected, of which 59 recovered and 14 died.

Following this line, Metschnikoff proceeded to study the relation of leucocytes to anthrax bacilli, to the microorganisms of erysipelas, and to various other affections. He likened specific inflammation to a warfare in which the invading army is represented by microorganisms, and

the resisting forces by the leucocytes. Even in details the analogy was maintained. Notice of the arrival of the invaders was telegraphed, so to speak, by the vaso-motor nerves; the line of communication, the avenues of mobilization, were represented by the bloodvessels. The aim of the invader is to secure the territory, to multiply rapidly, to live at the expense of his host, and to manufacture and circulate substances injurious to him. The aim of the resisting forces is to encircle the enemy, inclose him, digest him, and render him inert in battle. Many phagocytes die in the process, and, if in large numbers, the heaps of the slain represent pus; an abscess is a battleground densely packed with dead bodies. If victory remains with the invaders the organisms pervade the affected part, multiply, and induce conditions incompatible with the life of the part, or perhaps with the life of the entire organism. If the battle is with the host, the parasites are destroyed, perhaps not without loss, but the normal state is gradually restored. Practically, on this theory each organism is regarded as possessing a standing army composed of mesoblastic cells, capable of rapid reproduction and rapid concentration, one important function of which is to protect the organism against destructive agencies invading it from without. Certainly a most attractive theory, fully deserving the attention which it has aroused. On the one hand widely accepted, on the other bitterly assailed, the question is as yet far from settled, and to the position in which it stands I propose briefly to refer, and then to offer some results of my own observations upon a disease in which special facilities exist for the study of the problem.

Metschnikoff has studied a number of diseases, erysipelas, anthrax, relapsing fever, and tuberculosis, with a view of finding facts in support of this theory, and his communications within the past four years have been numerous and elaborate.¹ They have been so widely abstracted and so often referred to that I shall not occupy your time by entering into details, but will briefly indicate the chief points upon which he lays special stress in these different affections, and note certain of the observations which have been made by other workers.

In erysipelas the cocci are attacked first by the leucocytes filling the lymph spaces, which rapidly proliferate and actively eat the microorganisms. Not alone do the colorless corpuscles act as phagocytes, but the fixed connective tissue cells assist in an important manner. In cases of recovery he found that behind the advancing cocci the leucocytes were crowded with parasites which showed evidences of digestion and destruction. The connective-tissue cells do not appear to attack the cocci, but are chiefly concerned with the absorption of the inflammatory exudate, even taking up the leucocytes which have died. In fatal cases there was enormous development of micrococci, the majority of which lay free in the tissues not enclosed in the phagocytes. Inoc-

¹ Ueber eine Spisspilzkrankheit der Daphnien.

¹ Published chiefly in Virchow's *Archiv*.

ulations with erysipelas cocci in white rats confirm these observations made in man. The leucocytes attack the parasites, which undergo rapid degeneration in the protoplasm. The larger connective tissue cells, macrophages, did not attack the cocci. Metschnikoff recommends experiments upon these animals with the erysipelas cocci as an especially favorable field in which to study the struggle between the cells and bacteria.

In anthrax Metschnikoff has studied the relation of the phagocytes to bacilli introduced into frogs, which, as is well known, possess immunity at the ordinary temperature, but succumb when the temperature is raised. A graft of a piece of anthrax tissue under the skin of a frog is within from fifteen to twenty hours surrounded by leucocytes, which take up many bacilli. According to Koch, they may grow inside the cells and even burst them, but Metschnikoff holds that the anthrax filaments do not develop within the cell, but are gradually destroyed by them, and that this is the reason why the frog at an ordinary temperature recovers. In the heated frog the bacilli rapidly develop and the efforts of the leucocytes proving insufficient, the animal dies; not, it is asserted, from any inactivity on the part of the leucocytes but because the bacilli secrete a liquid which protects them from attack.

In Baumgarten's criticism¹ he relates some experiments with the anthrax bacilli which directly antagonize these observations. Pigeons do not die when inoculated with anthrax, and he found that the bacilli injected degenerate in precisely the same way in these creatures as when in distilled water; only here and there did the leucocytes contain the rods.

He found that in frogs, though the bacilli are eaten by the leucocytes inversely to the degree of heat to which the animal is exposed, there is never total destruction of the bacilli by the phagocytes.

Hess² has performed experiments which bear directly upon these points. Anthrax cultures in Zeigler's glass chamber, inserted beneath the skin in animals not very susceptible to the disease, as dogs and birds, showed active migration of the leucocytes into the chamber which appear to attack the bacilli and to destroy them. These very striking experiments certainly indicate what, of course, is well known, a high degree of activity on the part of the leucocytes, finding their way, as they do, into the chamber closed at all points except one narrow orifice. But, as Hess says, it is a question whether the disintegration in the cells necessarily means destruction by the cells.

In relapsing fever Metschnikoff states that the spirilli are not attacked by the leucocytes in the blood but are destroyed only in the spleen. In the artificially produced disease in monkeys, he finds abundant inclusion of the spirilli in the phagocytes of the spleen during the period of the rise in temperature before the crisis. This, however, may simply mean that the spirilli, most of which gradually disappear from the blood at the crisis, have lived their life and are about to die, and in this state are taken up by the normal splenic phagocytes, just as are the effete red blood-corpuscles. He explains the recurrence of the second, or even of the third, attack of the fever by supposing that certain spirilli remain alive after

the crisis and start afresh a new generation, which is not retarded in its growth, as the phagocytes are too busy in digesting the spirilli which they had eaten during the former attack.

An interesting study of phagocytosis has been made by Laehr, a pupil of Ribbert,³ who has studied the effect of injection into the lungs of rabbits, through the trachea, of staphylococcus pyogenes aureus. Within a few hours the cocci are almost all to be seen within the alveolar epithelium, and in the leucocytes, which latter, in the course of a few days, disappear from the alveoli and pass into the bronchi. Meanwhile, the alveolar epithelium proliferates, causing a catarrhal inflammation. Within the first week the cells contain many cocci, which gradually become less numerous, and in the second week entirely disappear. He looks upon this as a confirmation of Metschnikoff's views.

In a second investigation, Hess² has studied, in rabbits and in cats, the relation of the leucocytes to the staphylococcus aureus inoculated in the cornea. At first there is marked increase, which leads to an acute inflammatory process in the neighborhood of the cocci. The leucocytes increase rapidly, and within two or three days almost all of the cocci are within cells. In cases which recover, by the sixth day no cocci are found. When the process does not result in healing, the phagocytosis is slight.

Baumgarten states⁴ that experiments made in his laboratory do not confirm these results of Hess.

Ribbert, in his study on the destruction of pathogenic bacteria in the body, supports Metschnikoff's⁴ views. He found, after injection of the spores of Aspergillus and Mucor, that they collected in the organs of the experimental animals, particularly in the liver and lungs, and that within a few hours after injection they were surrounded by leucocytes, which either completely prevented or restricted the growth of the germs. Injections in very large quantities might not be sufficient to hinder the growth of the parasites, and the animal died. In the lungs and in the liver the phagocytes are much more active than in the kidneys. Precisely similar occurrences were found where the spores were injected into the anterior chamber, and it is worthy of note that he found on the anterior surface of the iris, in the neighborhood of the pupil, the phagocytes much more active and the disintegration of the spores much more marked than in the posterior part of the iris in contact with the lens. Ribbert holds that the destructive influence of the leucocytes is exercised chiefly by their preventing access of nourishment to the spores (particularly of oxygen), and in favoring, also, an accumulation about them of destructive metabolic products. He regards the fixed connective-tissue cells of the liver, and the giant cells which develop in the liver and in the lungs, as the most important agents in the final destruction of the spores.

As we might suppose, the views of Metschnikoff have met with sharp criticism in many quarters, and from no one more ably and at greater length than from Baumgarten.⁵ While not denying that the leucocytes eat the

¹ Zeitschrift f. klin. Medicin, Bd. xv. Hft. 1 u. 2.

² Virchow, Archiv, Bd. 209.

³ Abstracted by Bitter; Zeitschrift f. Hygiene, Bd. 4.

⁴ Virchow's Archiv, Bd. 110.

⁵ Jahresbericht, Bd. 3.

⁶ Abstracted by Bitter; Zeitschrift für Hygiene, Bd. 4.

⁷ Loc. cit.

bacteria, he claims that the process is by no means universal, and is carried on so unequally, that we can scarcely speak of an active warfare waged against the parasites.

As a specially weak point, he alludes to the powerlessness of the phagocytes in the *Daphnia* disease so soon as the conidia are formed from the spores.

In relapsing fever, the freedom from attack which the spirilli enjoy in the blood is urged strongly against the phagocytic theory. The fact that spirilli are found in a number of cells of the spleen toward the crisis simply means that the phagocytes of this organ behave to them as to other foreign bodies. Probably, too, the spirilli begin at this time to lose their vitality, as is shown by their less active movements, and are then readily taken up by the splenic leucocytes in a manner precisely similar to effete blood corpuscles.

In erysipelas, Baumgarten criticises the position in which Metschnikoff finds the parasites, namely, in the second zone, behind the advancing cocci, as conclusively showing that they are not fighters of the battle—not, as he expresses it, "the heroes of the day, but the hyenas of the field."

The observations of Christmas-Derkinck-Holmfeld¹ are also directly opposed to the theory of phagocytosis. He finds in anthrax experiments that very few of the bacilli are taken up by the leucocytes. In rats they degenerate within two or three days after inoculation, and for the most part outside of the cells. He holds that pus formation is a conservative reaction against the penetration of the bacterial germs, but that the neutralization of the action of microorganisms depends much more on the chemico-biological relations of the tissues than on any property of the cells to destroy them by inclusion. His view, in fact, approaches that of Ribbert already referred to, in ascribing the limitation of bacterial growth to nutritive changes, particularly to the restriction of oxygen, rather than to any phagocytic action of the cells.

In Flügge's laboratory, observations have been made by Bitter and by Nuttall,² of San Francisco, which directly contradict those of Metschnikoff. Nuttall's elaborate experiments appear to show conclusively that the destruction of the bacilli in the living body is not effected by the phagocytic action alone.

And, lastly, in tuberculosis, the question of the relation of the cells to the bacilli is being carefully studied. In his recent paper on the subject,³ Metschnikoff claims that the degeneration of the bacilli, which has long been known to occur within the giant cells, results directly from their phagocytic action, and is not a natural decay. Baumgarten, on the other hand, regards the relation of the giant cells to the bacilli as one of the strongest evidences against the theory of phagocytosis.

With the relations of phagocytes to bacteria, I have had so little practical experience that I hesitate to express any positive conviction on the question, but I have, for nearly three years, been working at a problem identical in all its relations, but in which the parasitic bodies belong to a higher class of organisms. I refer to malaria,

and to the hæmatozoa which occur in the blood of this disease. A sceptical attitude in these days of hasty observation and of still hastier conclusions is peculiarly appropriate. I complain of no one who, without ample opportunities for personal study, claims the right to question the full significance of Laveran's important discoveries. Perhaps better than any one else, I am in a position to extend sympathy to the sceptic, as, until ample material came to hand in 1886, I was among those who looked upon the work of Laveran with extreme incredulity. The corroboration in almost every detail which his studies have received during the past three years is in all respects remarkable. Working as he did, alone in Algiers, under circumstances the reverse of favorable, without proper laboratory equipment, without the stimulus to be found in the association of men in large cities, it is not only in the highest degree creditable, but most encouraging, that an army surgeon, actively engaged in the duties pertaining to his battalion, could accomplish so thorough a piece of work, requiring but little subsequent correction, and receiving at all hands ample confirmation.

Richard, in France; Marchiafava and Celli, Golgi and his pupils, in Italy; Sternberg, Councilman, James, Shattuck, and myself, in this country; and Vandyke Carter, in India, working far apart, have all practically confirmed, with minor modifications and amplifications, Laveran's observations.

While the invariable association of these parasites with malaria would appear to be settled, their precise morphological relations are still a matter of discussion. I have urged, from their resemblance to other hæmatozoa, that they should be classed with the genus *Hæmatomonas* (Metrophonow), which includes all forms of monads parasitic in the blood, of which a considerable number is known among the lower animals. Briefly, to refresh your memory, I may refer to the forms which occur in ordinary malaria. In the acute cases there exist in the red blood-corpuscles hyaline and pigmented bodies which undergo amœboid changes, and which gradually destroy the corpuscles, converting the hæmoglobin into a black pigment. Under certain circumstances, more particularly during the paroxysms, these bodies increase in size, and undergo segmentation, breaking up into a number of small free spherical bodies. In smaller numbers in the blood, but more constantly in the spleen, are the remarkable flagellate organisms. Lastly, in more chronic cases there are the still more extraordinary crescentic forms. Practically, the unanimity which exists in the statements of the observers above named regarding these bodies, places the question of their existence in malaria (and I may say, based upon the number of negative observations, in malaria only) beyond any reasonable doubt.

That they truly constitute the actual germ of the disease is, however, a point upon which opinions may differ. The constancy of their presence, their absence in other individuals in malarial regions, their abundance in the graver forms of disease, the destructive influence they exert upon the blood-corpuscles, are urged by Laveran in evidence of their pathogenic nature.

However this may be, the question which here concerns us relates to the relation between the phagocytes and these bodies. Surely one might suppose that here, if anywhere, the theory of phagocytosis might receive

¹ Fortschritte der Medicin, 1887.

² Archiv für Hygiene, Bd. iv. I would particularly recommend the summary of Bitter's to those wishing further details, and for a striking series of experiments, the paper of Nuttall's.

³ Virchow's Archiv, Bd. 113.

confirmation or rebuttal. What but phagocytes are the amœboid forms of these parasites which exist in the red blood-corpuscles, gradually destroying the stroma and the hæmoglobin until nothing but a shell remains? Here, indeed, are foemen worthy of the steel, or, rather, of the plasma, of the leucocytes. What, then, are the facts? How far can we say that in the blood in malaria, the seat most assuredly of the chief pathological changes, in acute cases, that there are evidences of a struggle between the phagocytes and the hæmatozoa. It has long been known that the leucocytes in this disease (particularly in chronic cases) contain pigment granules. There is no other affection in which melanæmia is so constant a feature, though it is now and then met with in other conditions. The leucocytes obtain the pigment either in the blood itself, or in the liver, spleen, or marrow, where the red corpuscles undergo their final destructive changes. In an examination of nearly one hundred and fifty cases of all forms of malarial affections, I have looked carefully at this point with a view of determining the exact mode in which the leucocytes obtain their pigment, and in my observations of the past two years the question of their relation to the various forms of the hæmatozoa has engaged my special attention. It may be remarked, in the first place, that there is certainly an increase in the number of white blood-corpuscles, an increase not associated, so far as I know, with any special change in the character of these bodies.

The result of my work in this direction may be stated in a few words. In the blood, at least, there is very slight evidence of the existence of phagocytosis. Here and there, it is true, we meet with leucocytes which have included the amœboid forms of the parasite, either free or still surrounded with the shell of a red blood-corpuscle. I have but three or four sketches in a whole series illustrating this fact. Occasionally a crescent may be seen within the white blood-corpuscle, more frequently the smaller free bodies which result from the segmentation. I have in my paper on this subject, given a sketch of a leucocyte which was watched for an hour and one-half and had included one pigment body, and was about to take a second, behaving identically though more deliberately than its great prototype, the pond amœboid. I confess myself disappointed in this respect. It may be urged that in a blood drop after withdrawal, the conditions are not favorable for study. But the absence of any great number of leucocytes containing parasites in a comparatively unaltered state, shows that certainly in the circulating blood the leucocytes do not actively attack and eat the parasites. More probably, I think, they pick up the pigment granule after the disintegration of the parasite, or in such regions of the circulation as the spleen or the bone marrow where the conditions are more favorable to phagocytic action. Even on the warm stage with the leucocytes displaying for hours amœboid movements, and in specimens which contained "foes" innumerable, it was exceptional to see evidence of active warfare.

It is, of course, more difficult to obtain evidence of the relation of the supposed contestants in the spleen, liver, and marrow, the organs in which regressive and progressive blood changes are constantly going on. I have not myself practised puncture of the spleen in these cases, as has been done extensively by Councilman. Fatal cases of malaria are not now very common. I

have only had opportunities of examining two, both of chronic paludal cachexia, the result of prolonged exposure in Panama. One, an old man, admitted under my colleague, Dr. Musser, whose blood presented many of the characteristic forms; the other, a profoundly anæmic man, with a greatly enlarged spleen, but in whose blood very few of the parasites were found. In both instances the liver, spleen, and bone marrow showed characteristic melanotic changes. In the spleen the pigment in various shades, from brown to deep black, was chiefly in the thickened trabecular tissues and about the vessels. Teased portions showed:

(1) Large numbers of leucocytes containing brownish-black pigment grains. A few of the leucocytes contained the small amœboid forms which had been noticed to be very abundant during life.

(2) Larger cells, containing red blood-corpuscles in all stages of degeneration. These cells were of various sizes, and contained a variable number of corpuscles, from eight to ten, or even more. Some of the red corpuscles contained amœboid parasites, but by far the larger portion of them presented the usual appearance met with in cells of this character in the spleen.

(3) Large, irregular, flattened cells, probably derived from the epithelium of the spleen capillaries, which contained granular black pigment, and occasionally red blood-corpuscles.

(4) Spindle or branch cells of the reticular tissue enclosing brownish or black pigment grains.

(5) Free pigment.

Practically the condition was similar, though more extensive in degree, to that met with in this organ and in other febrile states associated with extensive blood destruction. I really could not say that the splenic phagocytes exercised any selective power in picking out for attack those corpuscles which contained parasites or crescentic forms, which in one of these cases existed in considerable numbers. The bone marrow in both cases presented microscopic changes characteristic of the lymphoid tissue, and had a grayish-brown color, due to excess of pigment. There were in it large numbers marrow cells and ordinary leucocytes containing irregular pigment, and occasionally free amœboid forms of the parasites. The cells containing red blood-corpuscles were very abundant, but here, as in the spleen, it was particularly noted that the red corpuscles not containing the parasites were as frequently, or even more frequently, enclosed in the cells. In the liver the pigment existed in three elements.

(1) In leucocytes, which seemed numerous in the lobular capillaries, particularly at the periphery.

(2) In the walls of the capillaries, in all probability in the endothelial cells.

(3) In the stroma cells, particularly of the interlobular tissue about the portal canals, where in places it was sufficiently abundant to cause ordinary diffuse pigmentation, a part of which was due to grains lying free in the interstices of the tissue.

Metschnikoff states that in malaria the parasites are attacked chiefly in the spleen and the liver by the larger phagocytes existing in these organs, and to a much less extent by the leucocytes in the circulating blood.

We see, then, in malaria very little evidence in the blood favoring a theory of phagocytosis; certainly no such campaigning on the part of the leucocytes as might

be expected from the presence, in such numbers, of foes so destructive to the red corpuscles. In the spleen, bone marrow, and liver, the organs in which the dead or dying blood-disks are normally cremated, to use Weigert's expression, we have, as might be expected, an activity proportionate to the increased amount of material to be consumed, but scarcely such heightened phagocytic action as would indicate, on the part of the leucocytes, an aggressive warfare.

Into the interesting theory that to the action of phagocytes is due the immunity against certain diseases or against a second attack, I cannot now enter. In the present unsettled state of our knowledge it would be premature.

To conclude: While phagocytosis is a widespread and important physiological process throughout the animal kingdom, and while it undoubtedly plays a most important part in many pathological conditions, the question of an active destructive warfare waged by the body cells against the microorganisms of disease must still be considered an open one.

ORIGINAL ARTICLES.

THE EFFICACY OF FILTERS AND OTHER MEANS EMPLOYED TO PURIFY DRINKING-WATER.

*A Bacteriological Study.*¹

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WATER, even after having been exposed to various chances of contamination, is very generally drunk as it is received from lakes or rivers, wells or springs, tanks or hydrants; and, despite the most disinterested warnings, people are not ready enough to believe that clear, sparkling water may at times be the most harmful of beverages; that it can carry with it, and introduce into the systems of a portion at least of those who drink it, the immediate causes of various impairments of health. Polluted water may be agreeable to the taste and have no visible sediment, yet cause many deaths. Such water the unskilled are slow to suspect, and but for the advance in sanitary science and the warnings conveyed by those familiar with hygienic studies, pure supplies of drinking-water in cities would be much more of a rarity than they are.

At the Hygienic Congress in Vienna, in 1887, M. Brouardel stated that he knew of over sixty epidemics of typhoid fever which had been induced by the drinking of infected water.² From typhoid

fever thousands of deaths occur every year; and, although scientific physicians are constantly urging these facts, it seems to be necessary that the danger present itself in the form of an epidemic before the people fully realize the immense harm which may result from impure drinking water. The epidemic which ravaged Plymouth,¹ in Pennsylvania, four years ago, attracted great attention at the time, but the history of several similar "visitations" since then would indicate that the full value of this severe lesson has not been properly appreciated.

Water may be very highly colored, and yet, if freshly collected near its source, show no very large number of bacteria. Such is the case with the best of the peaty "juniper" water which I have examined in various parts of the Dismal Swamp of Carolina and Virginia. Although having a large proportion of organic matter, this water is reputed eminently wholesome, and an inquiry among a portion of those drinking this exclusively, appears to show an unusual degree of immunity from the manifestations of diseases which might be attributed to the effects of improper drinking-water.² On the other hand, the turbidity which is common at this season of the year in the streams furnishing the water supply of many of our cities, is almost always accompanied by a large number of bacteria, as in these bottles of water which you see here from the Mississippi and from some of the rivers flowing into the Atlantic. The melting snow and the rain, which wash into the streams an increased amount of organic matter, introduce with it other germs (perhaps pernicious), and bring more nutriment than usual to the microorganisms already in the water, thus increasing their vitality and numbers.

It is believed that, like most of the varieties of bacteria found in water, the still larger forms of animal and vegetable life, ordinarily existing in lakes and rivers, are not prejudicial to health. It is through the products of the decomposition of many of these, when their life has ceased, that they may become harmful.

Chemistry has long furnished a means of recognizing polluted water, even when it yielded no sediment visible to the unaided eye, and the presence of considerable quantities of chlorides, nitrites, and

¹ Read before the Section on Public Health of the New York Academy of Medicine, April 5, 1889.

² Dr. A. Ollivier: Rapport au Conseil d'Hygiène Publique (etc.), Paris, 26 mars, 1886. A valuable monograph on typhoid fever by a scientific practitioner. As different parts of Paris receive water from rivers of very different quality, the careful health reports from garrisons and institutions, which vary only in the purity of the water supplied, are very instructive.

From the abundant evidence as to the influence of polluted water in inducing typhoid, may be cited the report of the Zurich Water Commission of 1885.

¹ Biggs and Breneman: N. Y. Med. Journal, vol. xli. (1885), May 23 and June 6. French and Shakespeare: *ibid.*, June 13. L. H. Taylor: THE MEDICAL NEWS, vol. xlii. (1885), May 16 and June 20.

² If harmful bacteria be introduced into it, such water favors their increase, while they would not be so apt to multiply in a very pure water having much less organic matter. Despite the good reputation which this water has had, and its wholesomeness near Lake Drummond, one who has seen the whole length of the canal down to the lower end, from which the supply for vessels is taken, can realize that the water may undergo considerable contamination before it reaches that part. I am sure that the present use (in our navy) of distilled water, kept in proper receptacles, is safer than the employment of the swamp water.

ammonias, determined by the chemical tests, has shown that many a water had more organic impurity than was permissible; and, when far below the accepted standard in this respect, such a water is very justly condemned. Yet it seems as though enough of the infective element of cholera or typhoid to diffuse an epidemic throughout a community can be added to a drinking-water and not be detectable by chemical tests. A perusal of the very complete and instructive paper by Professor J. W. Mallet (in the report of the National Board of Health for 1882) shows how widely such determinations may vary and how unreliable chemical standards may be.

The known disease-producing bacteria, however, do not appear to increase in water¹ in which there is not more organic matter than the proportion that chemists of constant and large experience regard as permissible in waters which they pronounce to be of fair quality, although these harmful bacteria, if once introduced there, may long retain their vitality and remain dangerous.²

Since, as yet, chemistry, when called upon to aid in solving the question of the purity of a water, can at its best only approximately estimate the amount of organic matter present and its state of oxidation, while it fails to inform us whether infective matter be present or not in a given water, it has been hoped that a sure means of recognizing whether a water be in any degree infected was reached when Koch introduced the bacteriological test. This test, while very definite and conclusive under favorable conditions, has thus far been able to decide absolutely in only a small portion of the cases of disease apparently coming from the use of drinking-water. Yet in those cases where the bacteria characteristic

of distinct disease have been detected in the suspected water, the isolation, culture, and identification of the disease-producing species furnish a convincing proof of the great value of the method.

The method employed is a simple one. Usually one cubic centimetre of the water to be tested is added to ten cubic centimetres of nutrient jelly, made by solidifying very nutritious beef-tea by the incorporation of one-tenth its weight of gelatine. Everything being done with rigid precautions, the water to be tested is added to the nutrient jelly, which has been softened by being warmed to a little more than 30° Centigrade. This is then quickly mixed and poured upon a cooled glass plate, where the fluidified gelatine soon becomes quite solid. Then the germs in the water, being scattered evenly throughout the mass, exercise their functions of rapidly multiplying at the room temperature; and, in the nutritious medium, the minute microscopic individual has perhaps by the third day become a dot as large as a pin-point, or much larger, containing thousands or millions of newly formed bacteria. These are called "colonies," and vary considerably in appearance, as will be seen by comparing the plates made with various waters. With a glass ruled into centimetre squares the number can be counted or estimated. If a large number of thousands be present, as in waters artificially prepared for tests and in sewage contamination of water, the only way to arrive at an accurate estimate is to employ in the eye-piece of a microscope a micrometer square, as first used, I believe, by Dr. Edw. K. Dunham, who combines with it the aid of an automatic counter. After determining the number we isolate and identify the species as in general bacteriological study.

There are many features of the subject which must be omitted in a paper of this scope, for they may not interest all, while the limits of the usual time allowed must be respected; and as the important points are to be discussed by eminent experts, I will not linger over the still unsettled question as to the absolute value of mere numbers of ordinary water bacteria in determining the sanitary merits of a water.³ It is, however, admitted that the fewer the varieties present, the less is the likelihood of sources of contamination affecting the water. In examining a considerable number of samples of water from various sources, I have observed that the less chance of contamination there was, the fewer bacteria have I found in the waters. Thus, fine mountain lakes and springs have never in my experience, unless

¹ In my tests, when I prepared an artificial water, I added the germs directly from a pure culture, unlike Bolton, and like Wolffhügel and Riedel (*Arb. aus d. Gesundheitsamte*, 1886, i. p. 455), with whose results, however, I cannot so well agree as I can with those of Bolton (*Ztsch. f. Hygiene*, 1886, i. p. 76). I have in but two out of many trials, with disease-producing species, found typhoid bacilli to increase in sterile pure water, and then only to the extent of thirty per cent.

² Chantemesse and Vidal (*Gaz. Hebdomadaire de Méd. et de Chir.*, 1887, pp. 146-150) kept typhoid bacilli alive for "months" in sterilized water from the Ourcq, which is the worst water supplied to Paris, and much impurer than the Croton, in which, when it was sterilized, I have been able to keep typhoid bacilli alive for only two weeks, and usually not over eight or nine days. I have observed that the greater the variation of temperature it was exposed to, the shorter time did this microorganism live in water. It has been found by all observers that oscillations of temperature around the freezing point were much more destructive to these than freezing them in water and allowing the ice to remain frozen. So, I have observed several times that of two flasks of the same typhoid water, the one which was kept at a temperature varying from 37° C. to 10° C., had, at the end of three days, many fewer than were in the flask kept between 16° C. and 10° C. In my last test of this sort, the flask of which the temperature varied most had, after two days, only a few more than a third of those in the one kept at the evenest temperature. Heraus: *Zeitschrift für Hygiene*, i., 2, p. 193. Hochstetter: *Arb. aus d. Gesundheitsamte*, ii., 1, p. 1.

³ Gärtner: *Correspondenz Bl. d. allg. ärzt. Ver. von Thüringen*, 1888; Nos. 2 and 3.

Centralbl. f. Bakt. und Par., iii., 5, p. 161 et seq.

Wolffhügel: *Arbeiten aus dem Gesundheitsamte*, Band i., iii., 5 Heft (1885), p. 546.

Plagge und Proskauer, *Ztsch. f. Hygiene*, 1887, pp. 470 and 486.

under very unfavorable conditions, shown as many as one hundred bacteria in each c. c., while the same quantity of water from a river draining thickly peopled valleys may show more than fifty times as many. While it offers less striking extreme differences than I have seen in the Hudson Valley and elsewhere, the Passaic water-shed may be briefly instanced as illustrating what I have just said. I give it because it is the smallest one near us offering every phase of importance in this connection, and because the subject of a supply to come to New York from that source has been seriously considered. The best of the upper lakes of this system showed on a pleasant day of the past winter only fifty-seven germs developing in the nutrient gelatine, while the Pequannock and other tributaries into which these lakes flow showed, in the samples collected as nearly at the same time as possible, over three hundred and fifty germs, which number increased as the water was taken further down stream; while at the Passaic Falls, in Paterson, over a thousand were detected in each c. c. Water from the hydrant in Newark, at the same time, showed nearly four thousand germs of bacteria in each c. c. The Newark water supply is at present derived from the Passaic, some miles below the place where sewage from Paterson is discharged into the river.

As in chemical, so in bacteriological water examinations, those who have had the broadest experience and who are most careful are best in a position to pronounce an opinion. Apart from any personal equation, there are still limitations to the entire comprehensiveness of the method. I may mention that bacteria are recognized as present in the human body and in its products and changed tissues, which bacteria are never found to live on the gelatine or other water plates. Then, too, a water may be submitted for examination when all the obvious infection has disappeared, or has become exceedingly diluted, and hence a most painstaking test then fails to reveal the presence of the characteristic micro-organism. The period of incubation, after the infection and before the symptoms are recognized, may in Asiatic cholera occupy the greater part of a week, and in typhoid often lasts for two weeks or more, so that all traces of the harmful bacteria may have left the water before it is suspected. By reason, also, of its likeness to various harmless species, and its tendency to be overwhelmed by other more rapidly developing forms, the "colony" of the disease-producing species may be hard to identify. In this respect typhoid is decidedly more difficult than cholera to recognize in ordinary water.

For those who believe that bacteria of certain kinds living in water, and with this introduced into the alimentary canal, can in many cases induce disease, this test, being so precise and delicate as it is, must appear the best way of determining whether the fil-

ters and other means in common use are efficient or otherwise in removing the dangerous elements from impure water passed through them—in short, whether they act as "disinfectants" of the water.

As various other agencies are used in conjunction with, or as supplementary to, filtration, it is proper that I here indicate how far they serve to purify drinking-water. One of the most prevalent means directly resorted to for this ostensible purpose is the admixture of wine (usually red). Adding this to an equal amount of Croton water, I find that a varying proportion of the bacteria are killed, sometimes less than half, even after the mixture has stood for days. The spirituous alcoholic beverages (having from 45 to 68 per cent. of alcohol), mixed with Croton hydrant water in equal parts, cannot be depended upon to kill all of the bacteria, especially when "earth" bacteria are present, and a small percentage of whiskey (less than 20 per cent.) has very little effect in this direction.¹ In malt liquors, as lager beer, the prolonged boiling in the brewer's kettle has destroyed the bacteria of the original water, even if they be of the most harmful and resistive species which we meet here. Various kinds, however, if present in vats, kegs, and bottles (because of imperfect cleansing), will live in beer.² Almost all the artificial mineral waters that I have examined are made of unsterilized water. Pressure of carbonic acid gas, and contact with it for days, reduces greatly the number of bacteria in a mineral water, yet the typhoid bacillus and others will live for days in Seltzer water. In New York I have found many bacteria in the few carbonated beverages that I have examined.

Of the various acids which it is occasionally safe to employ, chemically pure hydrochloric acid must be used in the strength of at least one-half of one per cent. to destroy all the germs in Croton water to which *B. subtilis*, *B. typhoid*, and other bacteria have been added; and may fail to sterilize the water even after hours of standing thus acidulated. Sulphuric acid is more than twice as potent, and usually in the strength of one part to five hundred of water

¹ Koch (Flügge, Chapter V., sec. 3) found that the spores of *Bacillus anthracis* were alive even after months of exposure to the action of absolute alcohol.

Pasteur and Joubert (*Comptes Rendus des Séances de l'Académie des Sciences*, 9 and 16 juillet, 1877) stated long ago that the spores of anthrax would remain alive in absolute alcohol.

Anthrax, however, need not be expected to occur in the water supplied here, and several recent experiments show that it is possibly destroyed by other bacteria which may occur in the water.

Anthrax spores are employed in such tests because they are considered the most resistant of all disease-producing forms of bacteria.

² The bacteria from this source which I have found in examining a limited number of beers were mostly of non-liquefying kinds, and there were notably many iridescent "spreading" colonies. [Jan. 1889.]

In these cases water from old surface wells was used to wash out the kegs.

will render this free from living germs. Sometimes, however, it has to be used still stronger, as it is in a certain extensively advertised nostrum, in which, under a fanciful name, it plays the principal part. Alum, lime, and the other chemicals which, by inducing sedimentation of organic and mineral constituents, carry down many of the bacteria in water, do not (as is often believed) insure the annihilation and destruction of organisms present.¹ It is the mechanical action of the film, which the presence of a minute amount of these causes to settle sooner than would otherwise occur upon sand in certain large sand filters, which insures their excellence, and the scarcity of bacteria must here not be attributed to a coarse chemical action. When chemicals are added in sufficient strength to destroy speedily all the bacteria which may be present, the water thus treated cannot be recommended as a beverage.

With the imperfection of all other methods, we have in prolonged boiling a sure disinfectant of any water. I have usually found that maintaining a water at the boiling point for even less than fifteen minutes sufficed to prevent any of its germs from developing in gelatine, and this even with bad waters²

¹ Residents of the Mississippi Valley, where the principle of settling water by adding alum has been extensively made use of, have expressed to me their belief that many digestive irregularities are caused by the use of alum. The danger lies in the use of an excess, and probably does not exist with the small amount which is permissible (2 : 100,000).

As was just noted of absolute alcohol, Koch also found that the spores of the *B. anthracis* were not destroyed by prolonged exposure to the action of a four per cent. solution of alum (or a concentrated solution of chloride of lime).

² The employment of silk threads, in and upon which are dried living bacteria with spores, seems to endow these germs with greater resistance to the action of steam heat than when the bacteria are (as in drinking-water) each surrounded by water. Having tested various waters from different cities, I was led to believe that heating for a very short time sufficed to sterilize them. So I have made several series of experiments on the spore-bacteria usually regarded as most resistant. I used Croton water, sterilized and not sterilized, and bacteria from pure cultures on agar and potato (where the spores were, just before the experiments, seen by the microscope to be very abundant). I added from these cultures a small amount to water in flasks. These flasks were well agitated several times; then several plates made to test the presence of living bacteria. The water was from these poured into sterile tubes and Erlenmeyer flasks plugged with cotton. Several of these were tested before and after the heating. A wire basket, holding eight of these cool test-tubes or flasks (each having from ten to twenty c. c. of the infected water), was placed in a heated steam-box. The temperature ranged from 93° upward, but the thermometer did not register above 100° C. These tubes were taken out as rapidly as I could cool the plates at once prepared from them (at intervals of from three to four minutes). While this is not so absolute as are some other methods, it seemed the best for the practical object aimed at. In this way I found that the spores of *B. anthracis* failed to develop on gelatine (in five days) after they had been boiled in clear water for from two to five minutes. That this result was not extraordinary will appear to those who consult a remark by R. Koch (and Gaffky and Löffler) in *Mittheilungen aus dem Gesundheitsamte* (1881), p. 322.

My results with *B. subtilis* are likewise not in accordance with the accepted views. I am still experimenting in this direction, but have never found any water bacteria (which would develop in nu-

from sewage-infected rivers. The process of distillation likewise sterilizes water.

That freezing temperatures destroy only a certain proportion of the bacteria which exist in the water from which the ice we use is formed, must, I think, be evident to those who have read Dr. Prudden's masterly paper on this subject. He there shows that the typhoid bacillus in ice retained its vitality for one hundred and three days. As he will discuss the subject this evening, I shall simply echo his previous warning against putting ordinary ice into water which we are to drink, as many kinds of bacteria may be thereby introduced.¹

The mechanical separation of impurities is the principal action in all filters, whatever their composition. The amount of oxidation produced by the passage of considerable quantities of water through inconsiderable masses of charcoal, sand, or anything else, seems never sufficient to chemically purify an infected water, although the Berlin system of filtration, by large beds of sand skilfully managed, effects a notable improvement in this respect.

Almost all tissues tend to separate visible particles from water which is turbid, or in which a deposit settles. The fact that such matter is retained from the water passed through filters appeals very strongly to most persons; and the general assumption that such visible matter is dangerous, and that its removal insures the purification of water, seems to explain the extensive use of filters. They are all composed of felt-like matter (filtrum), or of any tissue or substance so woven, composed, or arranged as by its structure to more or less completely arrest the passage of sediment and the floating particles existing in varying amounts in nearly all natural waters. The filters in domestic use may be ranged in two classes. In one class the water runs or is poured into a tank or other receiver over or beside some filtering medium, through which the flow is into a receptacle, and from this the water is taken as needed, or is stored as fit for drinking. Those of the second variety are designed to connect with the faucets, and to strain from the water such impurities as it brings through the hydrant.

One of the most widely used varieties is this simple

trient gelatine) after the water in which it was had been boiled for even a tenth of the time (six hours) which Globig states as necessary to kill (by steaming) the most resistant red-potato bacillus with which he experimented (Globig; *Ztschr. f. Hygiene*, iii., 2, p. 231). He used silk thread wrapped in filter paper.

¹ Prudden; on Bacteria in Ice, etc., *N. Y. Med. Record*, March 26 and April 2, 1887.

Bordoni-Uffreduzzi (*Centralbl. f. Bakt. u. Parasitenkunde*, i., 2 Band (1887), p. 495) insists that in natural (Italian) ice the bacteria which survive the extensive destruction caused by the cold of freezing do not gradually decrease, but that they indefinitely retain their vitality. He states that he agrees with Dr. Prudden's results in that disease-producing forms of bacteria resist freezing better than the harmless ones.

Fränkel—*Bakt. des Eises*—*Zeitschrift f. Hygiene*, i., 2, p. 302.

and portable one, which renders water clearer and more agreeable to the eye. It is composed of a cylinder of pressed carbon, three inches or less in diameter, and having nearly the same depth, which has from its interior a tube passing through the bottom of the funnel in which it rests, and thus requiring all the water passing out of the bottom of the funnel first to go through the mass of carbon. The flow is very sluggish unless use be made of pressure or suction, as, for example, by lengthening the tube taking water out of the filter, which impairs the effectiveness of this so far as purification of the water is concerned. After this has been used for a few days the retardation is very marked, and is due to the clogging of the filter by the various matters which form the sediment of the unfiltered water. This necessitates frequent cleansing.

Taking a freshly sterilized carbon filter of this sort and pouring into the funnel Croton water just drawn after considerable has run from the faucet, and then collecting in sterilized test-tubes the first flow from the filter, adding of this (as already explained) one cubic centimetre to ten parts of softened nutrient gelatine, the mingled gelatine and water being poured upon a cool plate, it is seen that, before three days have passed, several colonies have developed. Comparing the number of these with that found from the original Croton water plated just after being drawn, we find that at least from five to eight per cent. of the bacteria pass through the filter and develop. The percentage rises as more and more water flows through. From fourteen trials made with simple Croton water, I found that through a carbon filter, freshly sterilized and the introduction of germs from the air and other sources of contamination being guarded against, the first flow showed only a small percentage of the bacteria found in the original water used. Then I continued the use of the same filter without re-sterilizing, and endeavored in every way to have the conditions under which it was tested as much as possible like those which would exist in domestic use, and the supply of water in the funnel was allowed to become exhausted at times, as usually during the night.

In this filter thus used, the number of bacteria in the water passing through is, on the second day and thereafter, much larger than in the water taken after being poured into the funnel and before it has percolated through the carbon block. This excess, when large, seems to bear an inverse ratio to the volume of the flow.

It is a familiar fact that, if fresh hydrant water in a flask covered with cotton (to prevent the entrance of bacteria from the air) be allowed to stand for a day at the temperature of the laboratory, the original bacteria in the water may be found to have in-

creased considerably. So, as a collateral test, after water had been poured into the filter and before it had passed through the carbon, I have taken some of this original water and allowed it to stand in test-tubes and flasks, both stoppered and open, by the side of the filter similarly protected from the entrance of germs from the air, and exposed to the same variations of temperature.

This water, examined at the same time with that coming out of the filter, showed that the bacteria appear to multiply much more rapidly in the substance of the filter than outside in separate flasks, likewise kept at the varying temperature of the laboratory, as it then was. The water poured in rarely contained as many as 225 bacteria in each c.c., and yet in from one to three days from the time when the freshly sterilized filter had begun to be used, the water after passing through the carbon showed several thousand in every case, and at times more than 25,000 bacteria in each c.c.

All sorts of bacteria appeared to pass through, and not alone water species. The bacillus of typhoid fever will not only pass through, but in two of my trials I found that it had increased. For such special tests I used sterilized Croton water, into which bacteria¹ were introduced from pure cultures. For artificial typhoid-water the bacillus of typhoid was added from agar and potato cultures. Harmful bacteria can pass through such filters, can possibly increase in them under certain favoring conditions, and from parallel tests seem to retain their vitality longer when in the substance of the filter than when in a glass vessel beside it.

Clean sand fairly represents the granular masses of filtering material, and has all the advantages of powdered charcoal and magnesia as generally used for filtering considerable quantities of water, while it is more readily cleansed of the accumulated organic matter and, unlike spongy iron,² adds nothing objectionable. Unless, as in the case of the large Berlin filters and others acting similarly, rendered more effective by overlying silt and organic matter, beds of sand do not prevent the passage of large numbers of bacteria. I have found many bacteria in waters from various artesian wells coming from a deep stratum of sand. Through a thin sterilized

¹ *B. fluorescens* (Oneida) and *B. liquef. alb. aq.* (Croton). For cocci: *Staph. pyog. aureus* and a coccus obtained from the water of Salmon River, west of the Adirondacks. For a spiral form: *Sp. tyrogenum* was used as being harmless, yet similar to the cholera microorganism.

² For some of the reasons why spongy iron has fallen into disuse, especially in America, see a very instructive paper by the late Prof. W. R. Nichols in the Ninth Report of the Massachusetts Board of Health, 1878, p. 137.

For favorable notice of spongy iron, see Frankland, Proc. of the Royal Soc., No. 338, 1885. I do not care to consider it here, for it is a proprietary preparation as offered upon the market (in London), is variable in quality, and never sold here.

layer, from six inches to one foot in depth, of the finest sand procurable from beaches, all the way from forty per cent. to ninety-five per cent. of the bacteria in the original water pass, even when the level of the water is never more than three inches above the surface of the filtering mass. When a higher pressure is afforded by the water, very few of the bacteria are kept back.

As in nearly all other filters, so through a layer of sand all kinds of bacteria, harmless or harmful, appear to pass easily. Here, too, they can multiply in the moist sand. Thus, I have found the hydrant water, which entered the sand filter with 290 bacteria in each c.c., to come out through the filter (not freshly sterilized before using) with sixty and one hundred times as many as entered. In a gramme of sand taken from a filtering bed, I have found more than 10,000 bacteria.

The combination of other substances with sand, as in numerous small and large filters, fails to improve the usefulness of either, except in so far as the elimination of sedimental and other coarser particles is concerned.

Sponge is often employed for this purpose, as well as independently, yet it is incapable of separating bacteria from the water strained through it simply, or through sand or other substances used with it. When a large mass of fine, sterilized sponge is closely packed, the first few ounces of hydrant water that pass through may have a very small percentage of bacteria, but the proportion soon increases, while if the sponge-mass be considerable and loosely packed it may, even in the beginning and under exceedingly slight pressure of water, keep back no more than fifteen per cent. of the bacteria of the original water. This freshly sterilized sponge can at the end of the first day of ordinary use cause the water then flowing through it to contain from increase ten times as many bacteria as the Croton water poured in. The stronger the pressure, and the more rapid the flow, the lower is the ratio of excess due to increase in the filter over the quantity found in the unfiltered supply, which, of course, was here, as in all other tests, regularly plated at the same time with the filtered water. When the flow is sluggish, and when it stops entirely at times because of all the water supplied having passed through, the sponge substance favors the increase of the bacteria to a greater extent than is the case with sand. The sponge, within twenty-four hours after sterilization may, under these conditions, cause the water first running through after the intermitting of the flow to have five hundred times as many bacteria in each cubic centimetre as are found in the water supplied for filtration.

The only way really to cleanse such a filter is to remove the sponge and boil it. Then the process repeats itself. In a few hours after unsterilized

water moistens it, the mass of sponge is again teeming with bacteria. Sponge filters are, so far as I have seen, becoming less popular. It is easily recognized, even by an untrained observer, that the organic matter in sponge can undergo decomposition under the conditions obtaining in ordinary filters. Several of the large filter systems used in manufactories, hotels, and other places where large amounts of strained water are used, employ sponge and the filtered water abounds in bacteria.

Filter paper, as used in laboratories, is useful in separating precipitates and sedimental particles from water, but at least from fifty to seventy per cent. of the bacteria in Croton hydrant water go through with the water filtered, even if the pressure be exceedingly slight. The paper which I employed in the trials was, of course, carefully selected, and the folding was cautiously done, so as to prevent any break in the substance. Only single sheets were tested.

"Prepared cotton," cleansed as for surgical dressings, and so made absorbent, removes considerably fewer of the impurities of the water than does the filter paper. Two-thirds of the bacteria pass through when the cotton is at its best, freshly sterilized and carefully packed. If enough be used, it will usually render the water clear; but, as it has been lauded as a filter, I ought to add the statement of my observation that, when left moistened with water, as in the intervals of filtering through it, the bacteria of the original water can multiply over one hundred and fifty times, and all kinds can pass through it.

Combinations of the various substances mentioned have hardly any enhanced merit as strainers, and, as far as the bacteria are concerned, the combination seems to add no safeguard. Sand with carbon (bone charcoal) and sponge or cotton, hold back at first a larger percentage than sand would, by itself. Yet bacteria pass through in any case, and the increase afterward is greater than in simple sand. This I have seen in various filters.

In a well-known bottling establishment of New York, where immense quantities of artificial waters are prepared every week, all the water used in their preparation flows through a layer of fine sand in the bottom of an enormous vat and, from the receptacle into which it then flows, it passes through a layer of charcoal. These layers are each less than a foot in thickness, and water passes rapidly through. The sediment is thereby removed, but in the single examination which I was enabled to make I found that each cubic centimetre of the filtered water had over 3200 bacteria on each of the two plates made, while the Croton taken from the hydrant in the immediate neighborhood on the day before averaged a few over one-eighth as many (432).

Asbestos of the best quality, new, freshly sterilized and tightly packed, I have found capable of

holding back all bacteria when the pressure of the water was low, and the few that were forced through when the substance was defective did not seem to multiply so rapidly as in other filter-masses. While it, when of the best finish and most tightly compressed, deserves to rank next to the principle employed in porcelain filters, these latter are superior in that they can be regularly furnished of a uniform and definite quality which produces the best results, while I have found it difficult to keep the best asbestos boards up to their original standard of excellence, and if the surface be large enough to insure an abundant flow of water, flaws are liable to arise, and these let various bacteria through. With asbestos it is the fineness of the surface which the original water encounters that is important more than the thickness of the layer.¹

Although wire cloth strains out the coarser particles, its structure does not cause the removal of any of the bacteria from water passing through. Still it has the positive merit of being easily cleaned, and can be heated in an oven or boiled to sterilize it. Furthermore, it does not furnish a filter-mass for the bacteria to multiply in, although harmful bacteria can adhere to it and become dangerous if it be left uncleaned.

Closely woven cloth, such as thick, dense flannel, when only a slight pressure is exerted by the water, may stop ten per cent. of the bacteria in the water poured upon it. It is easily cleansed and, if very often changed, and boiled before using, it serves excellently as a simple strainer without the disadvantage, in this respect, of ordinary filters, in whose substance the increase of bacteria may be enormous.

Like porcelain, filters of porous stone, if of the best quality, prevent the passage of bacteria with the water filtered through them just after they have been sterilized. To test porous stone I employed selected perfect specimens of a popular filter which very satisfactorily clears all turbidity from as bad a water as that of the Hudson River at its worst. In these, the stone layer was cemented into a porcelain cylinder and was about three-eighths of an inch in thickness. The pressure was never more than that of ten or eleven inches of water, and, at the fastest, less than three-fourths of a litre passed through in an hour. As the sediment of the water settled upon the surface of the stone, the flow became much slower. During twenty-six trials of this kind of filter, it had previously been sterilized ten times by exposure to moist or dry heat, or to both, for several hours. In every one of these ten cases the various waters poured into the receptacle above the porous stone for the first time after sterilization, flowed through germ-free; that is, the stone was permeable for water, but at first allowed no bacteria

to pass through, and the plates of gelatine developed not a single colony from the usual cubic centimetre of the filtered water. After some hours of use, a few bacteria had insinuated themselves into the stone or were drawn through with the water. Within twenty-four hours the water flowing through and collected in sterilized receptacles contained many bacteria, which numbered regularly from seven to fourteen times as many as were in the original hydrant water supplied during these trials. After several days, the number of bacteria had so multiplied in the stone, that the first water running through after the stone had remained for several hours without any flow through it (as happens constantly over-night in domestic use), showed in each cubic centimetre of this filtered water over one hundred times as many as the average water poured in for filtration. To test further these filters, I have used artificial waters made by adding bacteria of all shapes and characters, from pure cultures to Croton water sterilized by boiling. These various kinds can get through, but the disease-producing and spiral forms usually died out in large numbers before any could pass. From Croton water, the most marked by its predominance in the filtered product was a short, mobile bacillus which was unlike the two varieties first forced through the porcelain in my experiments.

(To be concluded.)

SUCCESSFUL CASE OF NEPHRORRHAPHY FOR FLOATING KIDNEY.¹

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Miss E. J. F., of Sunbury, Pa., aged thirty-five, height four feet eleven inches, weight ninety-two pounds, was sent to the Woman's Hospital on October 4, 1888, by Dr. Mary A. McCay, with a diagnosis of floating kidney. For the following notes I am indebted to Drs. Chapin and McKee, resident physicians. The patient was delicate as a child; menstruation began at fifteen, and was always painful and irregular. At eighteen years of age she was thrown from a wagon, falling with considerable force on her chest and abdomen. Shortly after the fall she suffered from severe pain in the right side and a great deal of distress across the back. Menstruation ceased for six months, and was followed by dropsy and severe illness. There was frequent inclination to vomit, and a great deal of palpitation of the heart. In spite of constant medical attention, she dragged out a miserable existence. About seven years after the fall she noticed a movable tumor in the abdomen, which Dr. McCay believed to be a floating kidney.

Condition on admission.—Appetite and sleep poor; urine 1020 sp. gr., slightly alkaline, twenty-nine

¹ Hesse; Deutsche med. Wochenschrift, 1885, No. 5.

¹ Read before the Philadelphia County Medical Society, March 27, 1889.

ounces in twenty-four hours, no albumin, no sugar. Heart and lungs normal; uterus retroflexed. In the right abdomen was a tumor, about the size of the kidney, which could be freely and easily pushed two or three inches to the left of the middle line back into the right lumbar region, or down into the right iliac fossa. Neither the hilum nor the bloodvessels could be distinctly made out. Percussion over the position of the right kidney showed a tympanitic note, the left renal dulness being distinct and normal. The tumor was evidently not connected with the uterus, ovary, or liver.

Operation, October 26, 1888.—Ether was administered. An oblique incision was made at the outer border of the quadratus lumborum four inches long. So soon as the abdominal fat was discovered, search was made for the kidney. The colon was first found, but the kidney was absent from its normal position. Strong pressure being made on the abdominal tumor, it was partly pushed back into position, but could even then only be touched by the finger-tip. On separating the borders of the incision by retractors, it was seen to be the kidney, bare of all fat. In order to replace it entirely, it had to be seized by a volsella. Seven carbolized silk sutures were next introduced by a Hagedorn needle, four posteriorly and three anteriorly, through the capsule and substance of the kidney, by which it was attached to the muscles and aponeurosis of the abdominal wall. Seven deep sutures of chromicized catgut were then introduced through the entire muscular wall of the loin, but they were not tied, as I intended that the wound should remain open for a few days, if not permanently, in order to produce cicatricial tissue between the kidney and the muscular wall. No provision for drain was necessary of course. The wound was covered with an ample bichloride gauze dressing. Her recovery was entirely uneventful. Her highest temperature was 100.9°. The urine was entirely free from any blood, though the bladder was irritable, and the catheter had to be used for several days. The wound was so completely filled up within the first forty-eight hours, that I removed the stitches that had been passed through the muscular wall. I kept her flat on her back for four weeks, when she was allowed to rise for a short time. There was considerable, apparently rheumatic, pain in the small of the back for three or four weeks after the operation, which disappeared and again reappeared, and which seemed to be benefited by salol. Seven weeks after the operation there was a moderate amount of albumin in the urine, which disappeared after the use of Basham's mixture for three weeks. Soon after she got out of bed, I tried the effect of a pad to support the kidney in front, but its use caused so much discomfort that I abandoned it, trusting wholly to the silk sutures and cicatricial tissue to hold the kidney in place. The tumor formerly discovered in the abdomen was entirely gone, and the normal renal dulness reestablished, though a little lower down. She went home on the 5th of January, 1889. I heard from her to-day, March 16, 1889, and she says: "My back is still weak, but the pain is fast disappearing. The kidney is still firmly anchored, and I am feeling better generally. Words cannot

express my gratitude to you for what you have done for me."

REMARKS. *The cause.*—A lax abdomen following frequent pregnancies has been supposed to be the origin of floating kidney, as it is of floating liver. In the case here narrated the patient was unmarried, and the abdominal wall was not at all lax. Again, the absorption of the perinephritic fat has also been supposed to be a cause; but in this case, as soon as the abdominal wall was penetrated, the perinephritic fat was at once encountered. But it was a noticeable fact that the kidney itself was entirely free from any fat. In other words, the fatty bed in which the kidney should lie was in its proper place, but the kidney was displaced, and there was no fat on the kidney itself. It seems reasonable to conclude that the dislocation of the kidney was due to the fall at the age of eighteen, though the abdominal tumor was not discovered till seven years later. Landau, who has written the best monograph both upon floating kidney and floating liver, states that, of 314 cases of floating kidney, 273 were in women as against 41 in men. In 178 cases, it existed on the right side in 151, on the left in 13, and in 14 on both sides. The present case being in a woman, and upon the right side, emphasizes still further his statistics.

The symptoms.—Digestive disturbances, especially constipation and very fetid breath, were not marked, though they were present to a moderate degree. The chief trouble was pain and constant discomfort, which was not only physical, but mental, the very existence of the tumor being a source of constant worry. The tumor itself was not especially tender to the touch, but it created a constant aching pain. Neither the hilum nor the pulsating renal artery could be distinctly made out, but the character of the tumor and the altered renal dulness made the diagnosis quite clear.

The treatment.—Recumbency alone has been advised by Landau, but this seems to me altogether too expectant. Only the most sanguine could believe that by this treatment, if such it can be called, a kidney would resume its normal position and quietly continue there sufficiently long for the adhesions to be reestablished with any prospect of permanency.

I did not try any treatment by pad or bandage, as the patient was from a distance and could not remain the long time necessary to decide whether such palliative treatment would answer. On the other hand, extirpation of the organ was equally foreign from my thoughts. In my opinion, this should only be done after failure of an attempt at fixation. The danger to life of a floating kidney is absolutely *nil*. It is, therefore, only to remedy the discomfort that exists that we operate. Hence, I

do not think extirpation at all justifiable unless we first attempt to fix it *in situ*, and having so failed, it is only justifiable even then in case the discomfort is very great. Dr. Maurice H. Richardson (*Boston Med. and Surg. Journ.*, June 14, 1888), who has published an excellent paper with a full bibliography, quotes from Brodeur the following figures: Of 235 nephrectomies, 125 were done by lumbar incision, with 47 deaths (37.6 per cent.), 110 by abdominal incision, with 55 deaths (50 per cent.). As against this large mortality from nephrectomy, however, Gross has collected 17 cases of nephrorrhaphy, with only 1 death, a mortality of only 6 per cent. It should be added also, that in the fatal case (Ceccherelli, *Centralbl. für Chir.*, 1884, 44, 743) the surgeon passed the stitches around the twelfth rib, a procedure which is absolutely needless as well as dangerous.

Hahn (*Centralbl. f. Chir.*, 1881, p. 449) first proposed fixation for a floating kidney by operative procedure, and practically perfected the operation. The operation is simple. The patient being laid upon the side, an oblique incision is made at the outer border of the quadratus lumborum. The edge of this muscle being recognized, the perinephritic fat is found immediately in front of it, at its outer border. This fat having been cut or torn through, the kidney may be seen at once, but, if it is very movable, it may be so far displaced as not to be seen, or, as in the present case, may be even felt with difficulty by the tip of the finger, even when an assistant pressed it firmly back through the abdominal wall.

Mr. H. Morris (*Surgical Diseases of the Kidney*, p. 45) makes a distinction between a kidney which has no mesonephron but moves freely about behind the peritoneum, this being called a "movable kidney," and a "floating kidney" which does possess a mesonephron, and therefore floats freely in the peritoneal cavity. In cases, therefore, of a strictly floating kidney, it would be necessary to open the peritoneal cavity before it could be fixed in the loin. This distinction is confirmed by four cases of dissection to which Mr. Morris refers. Comparing them with the present one, the range of movement to the left of the umbilicus and into the right iliac fossa was so great in this case, that it would seem proper to call it a "floating kidney," yet, at the operation, no renal mesentery or mesonephron was found. The probable mode of its production would also militate against the existence of any mesonephron. The kidney was far away from its normal position, but when pushed back into its proper place no layer of the peritoneum could be found that by any possibility could be called a mesonephron, and the peritoneum was certainly not opened.

In spite of the fact that Paoli (*Centralbl. f. Chir.*, 1885, 51, 910) cut through the twelfth rib in order to obtain room, it would seem to be rarely necessary to do so. When found and pressed back, the kidney should be fixed as nearly as possible in its normal position. Usually it will be impossible to replace it as high as it was at first, but lowering the site by two inches is not uncommon, and seems to be of no importance.

The sutures that have been employed (either of silk or of catgut, disinfected, of course) may be passed (1) through the capsule of the kidney, or (2) through the parenchyma and capsule both, and may either be (3) left permanently or (4) removed. In this case I employed antiseptic silk, which I consider decidedly the best, and passed the stitches not only through the capsule, but through the parenchyma of the organ itself, three on the anterior surface and four on the posterior, stitching the kidney to the muscles, and what I consider more important, to the aponeurosis, which exists on each side of the incision. Finally, these stitches were not removed, but were left *in situ*. I believe with Sennson (*Centralbl. f. Chir.*, 1886, 824), that many failures have been due to employing absorbable catgut, to the avoidance of passing the stitches through the substance of the kidney, and to removal of the stitches, which in all cases I think should be left in, whatever the material employed. Sennson inserted as many as fourteen silk stitches, which were left in place and caused no trouble. The wound is best left to heal by granulation. I introduced a number of stitches to close the abdominal wall if necessary, but in twenty-four hours it was so filled up that it was evidently a needless precaution. The larger amount of cicatricial tissue that is produced by leaving the wound to heal by granulation probably fixes the kidney more firmly.

Another very important point is, that the patient lie flat on the back for at least a month after the operation, in order that the cicatricial tissue binding it in place may become thoroughly developed and firmly established. Even then, I would advise some support for the kidney in front by a pad or bandage, provided the patient bears it well. In this case I soon abandoned it, as it caused too much discomfort. It is to be noticed that though the stitches were passed through the kidney substance, the patient had no hematuria (this was carefully watched for) and that no inflammation or reaction seemed to follow. But seven weeks after the operation considerable pain developed in the region of the kidney together with some albuminuria. This disappeared, however, after the use of Basham's mixture. The pain seemed to be rheumatic, and was soon relieved by the administration of salol.

LOBULAR EMBOLIC PNEUMONIA.¹

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(Concluded from page 404.)

EMBOLIC pneumonia is usually bilateral; when single, the right and left lungs are attacked with equal frequency, in my experience, though in that of others the right lung is most often affected.

There is reason to suppose that embolic pneumonia may occur at any age of life, though in my eighteen cases I have only noted it between the age of fourteen and sixty-eight. In pyæmia it may, of course, occur at any period. In cardiac cases it is most apt to occur in middle life, presumably when cardiac compensation is breaking down, or when the system is beginning to show the signs of age. Among the antecedent diseases, or phenomena, were noted the following:

In eight cases endocarditis.

In eight, pyæmia, either from some fall, mechanical injury, or from operative procedures.

In three, pericarditis.

In three, chronic Bright's disease.

In one, chronic bronchitis.

Some of the conditions occurred jointly in one individual. The cardiac cases were mostly associated with mitral disease; the pyæmia with surgical operations on the pelvic viscera in the male sex.

In approaching the topic of symptomatology we are at once confronted by the fact that we have to do with a disease that exhibits few characteristic traits at the bedside; and that we are often debarred from an inquiry as to them, either by the coincident systemic disease, whose other manifestations are of vastly more importance; or because the patient is unable to submit to a physical examination, without which a diagnosis would be conjectural. Hence, embolic pneumonia has not only been an obscure topic clinically, but is likely to remain so for some considerable time. More than this, embolic cardiac pneumonia usually occurs in patients that are already suffering from the interstitial pneumonia of heart disease, with a considerable amount of bronchial catarrh, and often pulmonary oedema to some extent; so that an infarction would naturally be more or less masked. Add to this, that infarcts occur chiefly where respiration is very feeble, and we have an additional difficulty thrown in our way. Indeed, we have not a single pathognomonic symptom. Take, for example, the temperature. The rise is slight, or there may be none at all. Even in pyæmic cases we may not be able to tell whether a sudden rise is due to the pneumonia, or to some other more distant manifestation of the disease. In cardiac

affections there will be little or no rise of temperature when infarction occurs; when the infarct has been recognized as a foreign body, we have reason to suppose that fever will show itself. Sometimes in a cardiac case the temperature will be subnormal. If it rise, it will not pass 102° ; $101\frac{1}{4}^{\circ}$ is the highest I have known.

In reference to physical signs I have found that they were marked in *only* about fifty per cent. of my cases. In two, the regular signs were present, but there was little in the others on which to fix a diagnosis. In two, there were the signs of pleurisy only. In two prolonged expiration, in one of which there was vocal fremitus, in the other cough. In one there were "whistling or squeaking" sounds, supposed to be due to obstruction. In one there was fine crepitation, with mucous râles. Of course, if the area of consolidation is large there will be no difficulty in getting physical signs, especially as the infarctions are apt to be peripheral. Pain in the side was present in only three instances and colored sputum in two. The pulse rate is rarely high. I have found it as high as 120, but it is rarely above 100. The respiration will usually be hurried. It is most so in pyæmia and may reach 30 to 50 and even 60. In cardiac cases it is not so much increased. If the infarctions reach the pulmonary pleura, which is usual, there will be pleurisy. Hence this secondary affection may be looked for as a regular phenomenon, and it will facilitate the diagnosis, unless there is much effusion.

It will be apparent, therefore, that we should not base our diagnosis on any symptom or group of symptoms, but should take them all into consideration. Signs of uræmia should not be neglected. I found them present in exactly half my cases.

The causes of death I found as follows:

In 10 septic poisoning.

In 3 heart failure.

In 1 cerebral compression.

In 1 cerebral embolism.

In 1 exhaustion from diarrhoea.

In 2 the causes were indeterminate, though in 1 of these the patient had uræmia.

The prognosis varies according as the case is cardiac or pyæmic. In the cardiac variety we have reason to suppose that the process *tends* to ultimate resolution with recovery. In examining an infarcted lung that has shown the signs of protracted heart disease, there is always evidence to show that restoration is being accomplished; in fact, our clinical experience sustains this view, but a fatal issue may take place at any time, though the cause of death is not directly traceable to the respiratory tract. Usually the disease will last a long time, and indeed like the broncho-pneumonia of children and of adults, it is characterized by successive inroads upon the lung. It is, therefore, subacute in its course. In pyæmia

¹ Read at the meeting of the New York State Medical Society, February 6, 1889.

A TABLE SHOWING THE CHIEF PHENOMENA IN EIGHTEEN CASES OF LOBULAR EMBOLIC PNEUMONIA.

No.	Age.	Sex.	Month.	Locality of disease.	Extent.	Antecedent condition or disease.	Coincident condition or disease.	Supposed or possible cause.	Duration.	Physical signs.	P. in.	Sputum.	Pulse; respiration.	Urinary symptoms.	Nervous symptoms.	Gangrene or abscess.	Cause of death.	Remarks.
1	45	F.	Jan.	Bilateral	Chronic diffused nephritis; aortic stenosis.	See preceding column.	Cardiac.	Unknown	Uræmic.	Uræmic.	Cerebral embolism.	
2	20	M.	Jan.	"	Mitral endocarditis; chronic bronchitis; pleurisy with effusion.	"	"	26 days	Sweating; whistling sounds from obstruction.	..	Bloody.	Trace of albumin.	Heart failure	One infarct measured 1½ inches in diameter.
3	38	F.	Nov.	"	Disseminated.	"	"	"	1 day	Prolonged respiration; vocal fremitus.	Delirium or coma.	"	"
4	43	M.	Feb.	Right	Half of lower lobe.	Pericarditis; mitral stenosis; chronic Bright's.	"	"	3 days	Regular signs.	..	Bloody.	Pulse, 100-130	Albumin 10 p. ct. scanty water.	"	A case of unusually extensive disease.
5	68	M.	July	Left	Chronic endarteritis.	Chronic Bright's.	"	Fine crepitation; mucous râles.	No casts; trace of albumin.	Present.	Compression of brain.	Ulceration of first portion of ascending arch of aorta.
6	22	M.	Sept.	Bilateral	Injury to hand; pyæmia.	See preceding column.	Pyæmia.	7 days	Cough; prolonged respiration.	None.	Resp. 40-60	Delirium	Abscess.	Septic poisoning.	
7	33	M.	Feb.	Left	Urethral stricture.	Pyæmia.	"	2 days	Signs only of pleurisy.	"	"	
8	50	M.	Jan.	Right	Renal cyst.	Pyæmia.	"	2 days	Regular signs.	Pulse, 120-133	Albumin and casts	Delirium and stupor.	"	"	Pyæmia due to forcible stretching of rectum(?)
9	56	M.	Dec.	Bilateral	Explor. dilatation of bowel.	Pyæmia; chronic Bright's.	"	7 days	Uræmia.	Convulsions and coma.	"	"	
10	18	F.	Dec.	Right	Synovitis of knee from foreign body.	Operation.	"	None.	Delirium	Abscess.	"	
11	14	M.	Sept.	Bilateral	Disseminated.	Acute inflammation; mitral endocarditis.	See preceding column.	Cardiac.	3 weeks	Present.	Albumin	Exhaustion from diarrhoea.	
12	28	F.	April	"	Lower lobes.	Pericarditis.	"	"	10 days	Uræmia.	Delirium	"	
13	48	M.	April	Left	Upper lobe.	Pericarditis and aortic endocarditis.	Pericarditis with effusion.	Pyæmia.	P. 102 R. 42	"	Abscess.	Septic poisoning.	
14	19	M.	Oct.	Bilateral	Disseminated extensively	Blow on the ribs.	Pyæmia.	Cardiac.	Marked by pleurisy with effusion.	Present.	Bloody.	Albumin 5 per ct.	"	
15	38	F.	Dec.	"	Lower lobes.	Mitral disease; chronic Bright's; hydrothorax.	See preceding column.	Cardiac.	8 days	"	
16	...	M.	Nov.	"	Urethral stricture.	Operation.	Pyæmia.	"	
17	...	M.	Jan.	"	Blows upon chest.	Chronic diffused nephritis.	"	"	
18	29	M.	Feb.	"	Inf. rheumatism; endocarditis; alcoholism.	See preceding column.	Cardiac.	Scanty sputum.	P. 100 R. 24	

the result will be fatal in almost every instance, perhaps in all. It is bound to terminate in pulmonary abscess, if the patient survive the early stages of the disease. From such suppurations there can be no recovery, I fancy.

Our treatment of embolic pneumonia is usually unsatisfactory. Jürgensen in his famous monograph refused to discuss it.¹

But dismissing from our consideration the topic of pyæmic pneumonia, which obviously needs no discussion here, and taking up the cardiac form of the affection, it is plain that we do have a condition of the lung that will often appear before us for treatment, and which we shall have to care for.

It is true that the cardiac disease first enlists our attention, with its attendant dangers, heart failure, cerebral embolism, and possibly septic poisoning.

And hence we very properly direct our main efforts toward securing strength and rhythm for the heart's action, while we enjoin upon our patient the necessity for his observing a proper dietetic regimen, and seek to impress on him the necessity for regulating his manner of life so as to avoid all heart strain. Naturally, too, we seek to correct any constitutional vice that may lie at the root of his cardiac trouble.

And the success that attends these efforts of the intelligent practitioner will often cause the disappearance of the pulmonary complication, or check its occurrence. Hence treatment is important though it be indirect. But if the lung has been implicated to any great extent, the direct treatment that is applicable to areas of extensive consolidation finds here an appropriate field.

In the vast majority of cases the prognosis of embolic pneumonia in heart disease depends upon the nature of the primary cardiac disease. The pneumonia of itself seldom kills—though it may, by a hemorrhage.

In fact, an embolic pneumonic process of itself actually tends toward spontaneous resolution.

The following cases are instances of the disease:

CASE I.²—A gentleman of this city was taken sick with a fever that confined him to his room for four or five days; then feeling better he went down stairs but soon returned and took to his bed. During the first two weeks of his attack the temperature ranged from 98° to 100°; the pulse from 140 to 150, sometimes to 160. It was manifest that he was suffering from aortic stenosis. But the respiration was never embarrassed except when the patient sat up; there was no pain except over the spleen. After death the post-mortem examination revealed infarctions in the lungs, spleen, and kidneys, and suppurative foci were found in the meshes of the pia mater. This case was classed as one of the so-called infective or suppurative endocarditis. The cusps of the aortic valve were covered with vegetations. The pulmo-

nary symptoms were masked by those of the other viscera.

CASE II. (Case No. 12 in Table.)—A widow, twenty-eight years of age, entered the Presbyterian Hospital in December of 1877. She expressed herself as having been delicate for five years, having had a slight cough all the time. But for seven years she had suffered from her heart and eight years previously had experienced an attack of acute rheumatism. About three weeks before admission she was taken with a chill. Breathing was found to be short and rapid. Expectoration frothy, pulse 120. Temperature 101¼. Nausea, pain in the right side—no heart murmurs, but heart's action feeble. The urine contained albumin in large quantity. The patient died apparently of exhaustion. At the post-mortem examination it was found that there was a marked mitral stenosis of an extreme character, the orifice being button-hole-shaped and only ¾ inch in its longest diameter. The kidneys showed evidences of a slight degree of chronic diffuse nephritis. The liver was of the nutmeg variety. In both lungs there were nodules of embolic pneumonia; in the right one the size of a turkey's egg, with some smaller ones. Left lobe in a similar condition.

CASE III. (Case No. 9 of Table.)—This is an example of pyæmic or embolic pneumonia leading eventually to an abscess. M., a salesman, was admitted into St. Luke's Hospital in December of 1882, for stricture of the urethra near the meatus. It was a stricture of large calibre and was easily cut with Otis's dilating urethrotome. In the evening some little difficulty was experienced in passing a flexible catheter, and water to the amount of only fifty ounces was drawn. Ten minutes afterward the patient had a chill, on the following day the patient had a second chill after the catheter was used. The temperature had now arisen to 101° and there was delirium with convulsive twitchings of eyes and hands, but these bad symptoms ameliorated and the patient talked intelligently; on the following day he died cyanotic. At the post-mortem examination infarctions from the size of a pea to a marble were found in the left lung, with a small abscess in the lower lobe. In the middle lobe of the right lung were several small collections of pus. The patient had also chronic diffuse nephritis.

MEDICAL PROGRESS.

The Clinical Significance of Colorless Stools Unaccompanied by Jaundice.—DR. ANDREW CLARK, at the meeting of the Royal Medical and Chirurgical Society of London, held March 26, 1889, read a paper written by Dr. T. J. Walker upon this subject. After referring to the accepted views of the significance of clay-colored stools, the author gave particulars of two cases in which, during life, a persistent symptom was the absence of color in the feces, and in which the diagnosis made of obstruction of the pancreatic duct, with a healthy condition of the bile-duct, was confirmed by the necropsy. From these cases Dr. Walker concluded, first, that the formation of hydrobilirubin, the coloring matter of the feces, depended on the mutual reaction of the bile and pancreatic fluid, under

¹ Jürgensen in Ziemssen's Cyclop., vol. v. p. 259.

² This case is not included in the Table on page 435.

the influences met with in the intestinal tract; secondly, that in disease a deficiency of pancreatic fluid would, equally with a deficiency of bile, cause the pathological condition of colorless or clay-colored stools; thirdly, that since, according to the most recent physiological researches, that portion only of the colored constituents of the bile which had been converted into hydrobilirubin was excreted in the feces, while the unchanged bilirubin, bilifuscin, and biliverdin were absorbed, it followed that, if hydrobilirubin could not be produced without the aid of the pancreas, that organ must have an important rôle in regulating what proportion of the bile entering the intestines should be absorbed and what thrown off in the feces.

Dr. Walker then pointed out that these conclusions received confirmation from the records of other published cases, that Claude Bernard recognized that the pancreas had a part in causing the color of the feces, and that the state in which the bile pigments were found in the meconium of the fetus, while the pancreatic function was in abeyance, also accorded with these conclusions. He further pointed out that the fact of the pancreas influencing the excretion of the bile in the feces would, if accepted, reconcile the discrepancy between the clinical observation that certain drugs produced copious bilious stools, and the physiological observation that these drugs had little or no influence on the secretion of bile by the liver, and that the same fact would explain those hitherto inexplicable cases in which, with no evidence of arrest of the bile-secreting functions of the liver, or of obstruction of its ducts, the symptom of white or clay-colored stools was persistently present. In conclusion, Dr. Walker indicated the practical importance of the views he had endeavored to establish in the treatment and diagnosis of pancreatic disease and of all forms of bilious disorder.—*British Med. Journal*, March 30, 1889.

The Treatment of the Navel of the Newborn.—DR. A. BAGINSKY, at a meeting of the Society for Internal Medicine, of Berlin, stated that he had seen no good results follow the application of moist antiseptic bandages to the navel of the newborn, as recently recommended and practised by a number of gynecologists. He considers the application of carbolic acid as most dangerous, for only a small abrasion of the navel is necessary to cause toxic symptoms which generally terminate fatally. He himself uses as a bandage, cotton to which small quantities of iodoform have been applied.

The Treatment of Phimosis.—The treatment of phimosis was also discussed on this occasion. Two varieties of this affection exist. In one variety we have to do with a short, rigid prepuce with internal agglutination. Simple cutting without dilatation is the best treatment for these cases. In the second variety we have to deal with a very long prepuce, and frequently with suppression of the secretion, swelling, and the discharging of pus. For these cases Dr. Baginsky advises dividing the prepuce. In a case of pyæmia which came under the author's observation, the fatal termination could only be attributed to the obstinate refusal on the part of the patient to submit to an operation, thus giving the microörganism every opportunity to invest the body and to set up, in the various organs and extremities, necrosis and suppuration.—*Wiener med. Presse*, March 17, 1889.

Creasote in the Treatment of Tuberculosis.—DR. BOURGET, of Geneva, gives in detail the treatment of tuberculosis with creasote, as first described in the *Semaine Médicale*, No. 10, and as at present carried out at the Medical Clinic of Geneva. The object of this method is to saturate thoroughly the organism with creasote. To accomplish this, creasote is given internally, externally, and by means of inhalations. In winter, creasote is given internally with cod-liver oil, as follows:

R.—Cod-liver oil 500 parts.
Creasote 2 to 4 " —M.

From two to three tablespoonfuls to be taken daily.

In summer, when cod-liver oil is not well supported, creasote is given in the following formula:

R.—Creasote 30 to 45 grains.
Arsenate of sodium $\frac{3}{4}$ grain.
Malaga wine 1 $\frac{1}{2}$ pints.—M.

Two wineglassfuls daily at mealtime.

Besides this, inunctions are made every evening with the following ointment:

R.—Creasote 10 parts.
Lanolin }
Lard } aa 50 " —M.
Olive oil }

Make an ointment.

Finally, the patient wears day and night a permanent inhalation-apparatus, consisting of two small pipes, one for each nostril, through the centres of which runs tissue paper saturated with creasote.—*Wiener med. Presse*, March 17, 1889.

Therapeutics of Bisulphide of Carbon.—Some observations on the employment of bisulphide of carbon, made chiefly in the Purisima Hospital, Santiago, S. A., are published in the *Revista Médica de Chile*, and show that this unpleasant remedy may have more therapeutic power than it is usually credited with. It has occasionally been employed in this country for diarrhoea, and externally for ulcers. Sapelier also employed it with apparent advantage in typhoid fever, and it has been recommended in diphtheria and other diseases in which microörganisms occur. The Chilean cases referred to included one of acute and one of chronic dysentery, one of atonic dyspepsia, one of simple gastric ulcer, and one of typhoid fever. The dose given was generally about two ounces of a saturated solution of the bisulphide in water, mixed with half a tumblerful of milk or a little syrup, taken half an hour or so before meals. This very quickly relieved the pain and tenesmus. In the typhoid case, which occurred in a child of ten (not a hospital patient), whose condition was becoming alarming, enemata of a pint of water, containing about half a drachm of bisulphide of carbon, were given, in addition to the internal administration of iodide of potassium and kairin. The diarrhoea diminished, the stools rapidly lost their offensive character, the gurgling disappeared from the iliac region, and the patient made a good recovery.—*Lancet*, March 23, 1889.

Operative Treatment of Prolapse of the Rectum.—DR. J. MIKULICZ, of Königsberg, describes in the *Archiv für klin. Chirurgie*, vol. xxxviii., his method of dealing with

prolapsed rectum, and puts on record seven cases in proof of its safety and efficacy. Of prolapse of the rectum there are, it is pointed out, three forms. The first and simplest of these, prolapsus ani, is a mere ectropium of the mucous membrane of the rectum in which the other coats of the gut, especially the muscularis, are seldom involved. In the second form, that of true prolapsus recti, the protrusion consists of all the coats of the intestine and, like the invaginated portion in intussusception, presents on section two tubes of intestine one within the other, and in contact by their serous surfaces. The third form, to which the name is given of prolapsus coli invaginati, is really intussusception of some part of the colon, and may be readily distinguished from the other forms by the fact that the external layer is not fixed to the anus, and that the finger can be passed into the rectum by the side of the protruded mass.

In cases of chronic prolapsus recti, with ulceration and gangrene of the exposed mucous membrane, and a persistent tendency to further protrusion, excision of the protruded portion of the intestine is, according to Mikulicz, the most suitable treatment. The limited excisions devised by Dieffenbach and Dupuytren are, it is stated, often useless and do not guard against prolapse, and cauterization, though a milder proceeding, is still not free from risk and may cause stenosis of the anal portion of the gut. Partial excision and cauterization will, it is true, result in contraction of the relaxed and softened sphincter, and of the immediately superjacent portion of rectum, and the constricted and rigid extremity of the gut will, if the case be a mild one, resist further protrusion from above; but in too many instances the *vis a tergo* overcomes this resistance and the cicatricial tissue yields sooner or later to renewed prolapse of the rectum. It is necessary, he states, to shorten the elongated and displaced rectum, and, in correspondence with this indication, to resect a large portion of the prolapse. This is held to be the most rational course in all old cases of prolapsus recti, and particularly after relapse.

The operation described in this paper was first performed with success by Nicoladoni, and resembles in some respects one advocated by Esmarch, in which, before resection, the upper part of the prolapsed gut is fixed tightly by ligature to a bougie introduced into the rectum so as to prevent any exposure of the peritoneal cavity. Mikulicz, after having passed two sutures through the free lower end of the prolapse, in order to stretch and fix in required positions the gut, makes a transverse incision through the anterior layer of the prolapsed mass, about half an inch below the anal margin. The inner tube of the prolapse having been thus exposed, the serous margins of the outer and inner layers are brought together by sutures, in order to shut off the peritoneal cavity. The inner tube having been divided transversely along the whole length of the outer incision and the intestinal canal opened, the cut edges of the inner and outer tubes are then fixed together by a second row of sutures. The posterior surfaces of the tubes are then divided and sutured in like manner, and the whole of the prolapse thus detached. In most cases the peritoneal sac does not descend between the layers of gut behind, the space being usually occupied by mesocolon which contains numerous vessels. In this operation Mikulicz insists on the use of silk in preference to catgut for the material of the sutures.—*Lond. Med. Recorder* March, 20, 1889.

Tait's Operation for Ruptured Perineum in Russia.—In the *Meditainskoï Obzrenië*, No. 16, 1888, p. 287, Dr. Vladimir v. Üspensky, of Moscow, published ten cases of perineoplasty for partial perineal rupture, in which he performed Lawson Tait's operation slightly modified by himself and Professor Snegireff. The modification consists in (a) the formation of a regular elongated quadrangular wound by making a semilunar or curved, instead of a straight, transverse incision into the remnants of the perineum; and (b) in inserting sutures through the skin instead of through the raw surface, at some distance from the cutaneous edge. In no case was there more than a trifling amount of bleeding. The operation, including the insertion of sutures, only occupied from five to twelve minutes. The sutures were removed between the eleventh and fourteenth day, when the wound was invariably found healed by first intention. The results left nothing to be desired. A solid and generally perfect perineal body was obtained in every one of the cases. Dr. Üspensky emphatically declares that Lawson Tait's method is the best in every respect, its advantages being extreme simplicity, rapidity of performance, and uniformly excellent practical results. He believes that it will supersede all other perineoplastic operations, at least in cases where no special indications are present, such as excessive tenderness of scars, demanding excision, etc. The same opinion was expressed by Professor v. F. Snegireff at a meeting of the Moscow Obstetrical and Gynecological Society.—*British Med. Journal*, March 30, 1889.

Treatment of Jaundice from Retention.—DR. LE GENDRE gives in the *Concours Méd.* the following treatment for this affection:

1. *Absolute milk diet*, consisting of five pints of milk taken pure or in an alkaline medium, in quantities of eight ounces every two hours.

2. *Intestinales antiseptics*, for which the following powders are to be taken:

R.—β Naphthol (finely powdered) . . . 23 grains.
Salicylate of bismuth . . . 15 " —M.

Divide into ten powders; one to be taken every time that some milk is taken.

3. *Sponging the body* every morning and evening with cold water, to which the following has been added:

R.—β Naphthol . . . 1 part.
Water . . . 5000 parts.

Heat and dissolve, filter and allow to cool.

4. *Purgatives* of a sodic base (such as sulphate of soda, etc.), to be taken every third day.

5. *Inhalations of oxygen*, or air-baths, if circumstances permit.

Although insomnia is often a troublesome symptom in this affection, it is best not to give any hypnotics, especially not morphine. Sleeping can frequently be induced by calming the troublesome itching with the following lotion:

R.—Sublimate } aa 5 grains.
Chlorhydrate of ammonium }
Camphorated alcohol . . . ʒi.
Cherry-laurel water . . . ʒiixss.—M.

—*Revue de Thérapeutique*, March 15, 1889.

THE MEDICAL NEWS.

A WEEKLY JOURNAL OF MEDICAL SCIENCE.

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SATURDAY, APRIL 20, 1889.

THE DIURETIC ACTION OF CALOMEL.

ALTHOUGH the medical profession is wont to hold itself as being a little better in respect to general knowledge, at least in the line of its own labors, than many other occupations, it is worthy of remark that almost any one can gain notoriety in its ranks at the present day, simply by turning over the pages of long-forgotten medical lore, and from them resurrecting some remedy, which in the hands of our ancestors often worked for good; particularly is this true if he be so conscienceless as to claim it as his own discovery.

While the diuretic action of calomel is not perhaps an instance of this kind, the practical lesson to be learned is the same. Calomel as a diuretic was one of the most frequently used of drugs for dropsy by Dr. J. K. Mitchell many years ago, and was also a standard remedy in the teachings of Dr. George B. Wood, yet at the present time clinical and experimental studies without number are published upon the subject, as if calomel was as new as erythrophlein. In a manner closely allied to this, Bergeon's treatment of phthisis arose, although our grandfathers for years used sulphur and sulphur waters in the treatment of such respiratory ailments.

Turning aside from the historical to the clinical facts, which, after all, must attract our attention, we find that at present the mass of modern evidence is so greatly in favor of the value of calomel as a diuretic that it cannot be denied by the most con-

servative. The researches of JENDRÁŠIK (*Arch. für klin. Med.*, Bd. 38, 1885, p. 499), of Wolf (*Virchow-Hirsch Jahresbericht für 1887*), and an innumerable number of others, have all reached results which, as a general rule, are singularly given to praise.

In the *Centralblatt für die gesammte Therapie*, for February, 1889, PAL, of Vienna, gives us still another report of a favorable character. Like many of his predecessors in this field, he has simply used calomel in cardiac dropsy, or more rarely in renal dropsy, and in the accumulation of fluid which results from cirrhosis of the liver. The results obtained by him are given in tabular form with the reports of the cases, and are as follows: In case number one, the man, who was twenty years of age, had a history of rheumatism in early life followed by cardiac disease which manifested itself in the course of about one year. Later he became confined to bed and was somewhat improved for a short time in his general condition by the use of digitalis. Finally, pain in the region of the liver, cardiac palpitation, dyspnoea, and anorexia asserted themselves, and a physical examination showed an icterode conjunctiva, a marbled skin, slight oedema of the feet and a double mitral murmur. The liver and spleen were both enlarged. Digitalis again acted favorably, but, nevertheless, calomel was soon added, opium being given to prevent intestinal overaction. As Pal's table shows with great clearness, the use of the mercury increased the urinary flow every time it was given, and also that the stoppage of the calomel, leaving digitalis to act alone, decreased the urinary flow to one-half or one-third that caused by calomel. Considering the increased quantity of liquid given off, the specific gravity remained fairly high, pointing to the presence of an increased amount of solids. Along with these changes there was marked general improvement.

The second case is illustrative of the fact that this calomel treatment is of very much less value in those cases in which there is degeneration of the heart muscle and cardiac infiltration. The patient was a man of forty-seven years, in whom cardiac palpitation, dyspnoea, and swelling of the feet with icterus was present, associated with loud murmurs at the apex and aortic cartilage, and who had an enlarged spleen and liver and albuminous urine. Calomel was used with little result. The man died and at the autopsy there was found aortic insufficiency and dilatation of the right heart. There was also universal dropsy and bilateral hydrothorax with chronic

endarteritis and occlusion of the coronary arteries. It is useless for us to go on detailing the states of the patients treated by Pal by calomel. Those who are especially interested should go to the original paper for further details. Suffice it for us to state that he gives us a profusion of tables showing, perhaps, even more forcibly than we have done, that the mild chloride of mercury is a powerful diuretic. Thus, in one instance in which the quantity of urine was only 800 c.c. in the twenty-four hours under the influence of the acetate of potash, calomel in small doses increased it in the course of four days up to 10,700 cubic centimetres, there being simultaneously a decrease and disappearance of the albuminuria which had been present.

Studies such as these are worthy of record and careful reading, as well as trial, and we are only sorry that more evidence of how calomel acts under these circumstances is not forthcoming. It is to be remembered, of course, that whenever any sign of intestinal disturbance is produced by the mercury opium should be used, although in any event the quantity of calomel given should be below that which is capable of causing ptialism.

DR. SAMUEL W. GROSS.

THE death of Dr. Gross, on the 16th inst., leaves a sad blank in the profession of Philadelphia, and is a grievous loss to American surgical science, of which, as his father before him, he was a distinguished ornament. To be thus struck down in the prime of life, in the midst of his work, and so full of zeal and energy, accentuates the shock of his death and deepens the sense of our bereavement.

He well maintained the strong reputation of his name, and every year brought him increasing fame as a thinker and writer in surgery. Carefully prepared, judicial, and systematic, his contributions were of solid worth and will have an enduring place in our literature.

As a teacher Dr. Gross had reached an enviable position, and his didactic and clinical lectures were models of clearness and precision. Of him, as of his father, it may be written, he taught principles—no slight merit in these days of devotion to technique.

Physically well endowed, there were in his nature exceptional qualities of mind and heart—a genial *bonhomie* and frankness and a warm, hospitable disposition which made his home the natural rendezvous of strangers in Philadelphia—qualities which

now leave mourning an exceptionally wide circle of friends in this country and abroad.

To his family and colleagues we extend our warmest sympathies in their affliction. In our next issue we shall give a full notice of his life and work.

MEDICAL EDUCATION OF WOMEN.

FOR some years McGill University, Montreal, has had a well-equipped Arts Department for the education of women, and not a few ladies have succeeded in taking the degree of B.A. The women now ask that the Medical Faculty admit them to their classes and are stirring up their friends to bring this about. The Medical Faculty object to mixed classes, and although not very enthusiastic on the subject, hint that a very large endowment (\$400,000 to \$500,000) might induce them to consider the subject of separate education. The ladies interested are busily engaged in collecting an amount of money for this purpose, and so far have been fairly successful. Public meetings have been held to promote the scheme, and the press is strongly in favor of "giving the ladies a chance." There are already two colleges in Canada for the medical education of women, viz., one in Toronto, and one in Kingston, Ont., and many medical men think these are quite capable of supplying the immediate needs of the country.

A PRELIMINARY programme of the General Sessions of the American Medical Association, Newport meeting, has just been issued. The President, Dr. W. W. Dawson, of Cincinnati, will deliver the Presidential Address, on the first day, Tuesday, June 25. The Address on Medicine, by Dr. William Pepper, of Philadelphia, will be delivered on Wednesday; the Address on Surgery, by Dr. P. S. Conner, of Cincinnati, on Thursday; and the Address on State Medicine, by Dr. W. H. Welch, of Baltimore, on Friday.

THE Shelby County, Indiana, Medical Society held a most successful anniversary meeting at Shelbyville, on April 8th. It was largely attended, and there were present several prominent medical men from a distance.

THE Third Congress of the German Society of Gynecology will be held at Freiburg, June 12 to 14, 1889, at the Woman's Clinic of the Grand-Ducal University. The following are the subjects for discussion: Autoinfection; referee, Dr. R. Kalten-

bach, co-referee, Dr. H. Fehling. Treatment of Extra-uterine Pregnancy; referee, Dr. Werth, co-referee, Dr. G. Winter. In addition a number of valuable papers will be read.

AN International Congress of Otology and Laryngology will be held at Paris from the 16th to the 21st of September, in the palace of the Trocadéro. Professor Duplay is President of the Committee of Organization, but all communications should be addressed to the Secretary, Dr. Loewenberg, Rue Auber, 15, Paris.

A CONSIDERABLE number of English and American physicians having accepted the idea of a Continental Anglo-American Medical Society, a meeting was held in Paris on March 14, 1889, at which the following gentlemen were present: Drs. Barnard, Boggs, Bull, Chapman, Darcus, E. Dupuy, Faure-Miller, Hon. A. Herbert, Jennings, Linn, Loughnan, Murray, Nachtel, Warren-Bey, and Webb. In addition to the above Paris practitioners, about forty from other places on the Continent have written to express their approval of the project, and many more are expected to send in their adhesions. At this meeting it was resolved to form a society under the above title, limited to British and American physicians residing abroad, the first general meeting to take place in Paris, on Monday, September 30, 1889, to be followed, in the evening, by a public dinner.

An Executive Committee was appointed to study the scope of the Society, to draw up articles of Association and By-Laws, and to submit suggestions, all to be voted upon at the first general meeting in September.

It was further decided to invite a certain number of distinguished members of the profession to become *Honorary Presidents* of the Society. The following names were proposed: Sir Spencer Wells, Sir Joseph Lister, and Dr. Richard Quain, for England; Drs. Fordyce Barker, Weir Mitchell, and J. S. Billings, for America, and Professors Ball and Brown-Séquard for Paris.

The following gentlemen compose the Executive Committee: Drs. Hon. Alan Herbert, Faure-Miller, Chapman, E. Dupuy, Barnard, and Linn, the last named being the Secretary.

At the semi-annual conversational meeting of the Pathological Society of Philadelphia, to be held next Thursday evening, 25th inst., Prof. Roswell Park,

of Buffalo, will read a paper entitled "A Study of Acute Infectious Processes in Bone."

WE regret to learn that Dr. R. C. Word, of Atlanta, editor of the *Southern Medical Record*, lost, by a disastrous fire on March 16th, his home and its contents, including his lecture notes and the MS. of a work nearly ready for the press, together with other valuable papers, the accumulation of years. We are sure that we are echoing the feelings of his colleagues of the medical press in expressing the warmest sympathy for Dr. Word in his great loss.

MRS. TURNER SARGENT, daughter of Dr. Oliver Wendell Holmes, has bequeathed \$5000 to the Massachusetts General Hospital, \$25,000 to Harvard College for the general purposes of the University, and the further sum of \$10,000 as a memorial of her father, the income to be applied to the use of the anatomical department, under the direction of the professor of anatomy. In the event of her brother, Judge Holmes, leaving no issue, she further bequeaths \$25,000 to the Boston Medical Library Association.

GEORGE TIEMANN & Co., of New York, have just published *The American Armamentarium Chirurgicalum*, a handsomely printed quarto volume of 862 pages, being a descriptive illustrated catalogue of surgical instruments and appliances, with a brief account of their uses and prices. The book must prove extremely valuable to the practising surgeon. It can be obtained, we are informed, upon the payment of a small sum, about covering the cost of binding.

SOCIETY PROCEEDINGS.

PHILADELPHIA COUNTY MEDICAL SOCIETY.

Stated Meeting, March 27, 1889.

THE PRESIDENT, W. W. KEEN, M.D., IN THE CHAIR.

DR. L. W. STEINBACH reported the following

CASE OF NEPHRECTOMY.

July 17, 1888, Mrs. Anna H., forty-four years old, came to the Polyclinic, complaining of indigestion, frequent vomiting of food or of mucus, attacks of palpitation of the heart, and loss of flesh. One week before attention was directed to an induration in the right hypochondriac region.

She considered herself in good health until eleven years ago, when she became dyspeptic. Five years ago she noticed a lump in her abdomen, and until one month ago she was able to attend to her household duties. She complained of headaches and constipation, and had not

noticed any sediment in or discoloration of the urine, nor could she recall having suffered with pain that would indicate the passage of a biliary or renal calculus. Pulse, respiration, and temperature normal, looked anæmic, complexion muddy, body emaciated. Inspection showed a prominence in the right lumbar region, whilst percussion and palpation revealed the presence of a tumor extending from the lower border of the ribs vertically for about seven inches, and, laterally, occupying the centre of the lumbar region to the extent of three inches. Percussion dulness was continuous with that of the right lobe of the liver. The tumor was freely movable below, and felt through the thin abdominal walls imparted the sensation of a bag filled with small pebbles. Believing that the case was one of a gall-bladder filled with calculi, and fearing lest manipulation would cause rupture of the cyst, he desisted from further palpation, and directed inquiries toward finding other symptoms of biliary obstruction. He drew off the urine with a catheter and submitted it to a chemical analysis, which showed the absence of albumin and the presence of some bile-pigment. No particulars of the nature of the stools could be obtained. He recommended a cholecystotomy.

On July 26th, an incision three inches in length was made in the right linea semilunaris, over the most prominent portion of the tumor, beginning at the border of the ribs and dividing the abdominal muscles and peritoneum; the apex of the tumor was reached without encountering any of the abdominal viscera. The calculi could now be felt more distinctly; an incision was made, and a few pieces of calculus removed, which, however, did not correspond in shape, color, and general appearance to calculi of biliary origin, and the forceps grasped a stone evidently of large size and immovably fixed.

The idea of impacted gallstones was dispelled—the tumor was a kidney.

Fear of rupturing the normal gall-bladder vanished, the lips of the abdominal incision were drawn apart more freely, and brought to view the margin of the right lobe of the liver and a normal gall-bladder in normal position. Further examination showed the tumor to be the right kidney distended by several calculi of different shapes and sizes. The organ was twisted by being turned upon its vertical axis from behind forward and to the left, and upon its horizontal axis from above downward and from behind forward, so that the dorsal surface and the upper end presented at the anterior abdominal wall.

It was at once decided to remove the kidney, a superficial examination indicating the existence of a kidney on the left side. The pedicle, consisting of ureter, artery, and vein, was ligated *en masse* with a silk cord, the kidney cut off, the abdominal incision closed with sutures and dressed. The patient, after half an hour, came from under the influence of the anæsthetic. Three hours after the operation the bladder was catheterized, but no urine obtained. Catheterization was repeated at intervals of six hours during the two succeeding days with a like result. The temperature at 8 o'clock P.M., six hours after the operation, was 101° F., falling to 99° on the following morning, gradually rising to 103° toward evening, and falling in the same manner to 100° on the morning of July 28th. She slept for a few hours during the night after the operation, and after a small dose of morphia was given without his knowledge, took moderate amounts of nourishment and some stimulants. About noon of the

third day began to complain of soreness all over the body, became irritable and restless, but continued to take milk and whiskey. She passed no urine up to the time of her death, which occurred at 6.20 P.M., fifty-four hours after the operation. A post-mortem examination was not held.

DR. J. H. GROVE then presented the specimens from

A CASE OF MALIGNANT TUMOR INVOLVING THE KIDNEY AND SUPRA-RENAL CAPSULE.

The appearance of the tumor indicated that it sprang from the supra-renal capsule; extended to and involved the left kidney, and that the supra-renal capsule was the seat and origin of a large cyst which was filled with grumous fluid and fatty disintegrated *débris*.

DR. W. W. KEEN then reported

A CASE OF NEPHRORRHAPHY

(see page 431),

and also a case of

UNCOMPLETED NEPHRECTOMY.

G. M. C., aged sixty-eight years. On April 6, 1886, had an attack of retention of urine. Violent expulsive efforts forced out a clot. The bleeding continued two or three days. With this he had pain in the right lumbar region. A month later another similar attack occurred, amounting to a distinct renal colic. Other attacks occurred in July, 1886, and in January, September, and November, 1887. After the last one, for several weeks he had repeated and nearly continuous hematuria with a sensation of heat in the right lumbar region, and he lost strength and appetite. January 14, 1888, he was taken extremely ill with pleuro-pneumonia and septicæmia. Both legs were attacked with phlegmasia. The dulness in the right kidney was increased, but no pus was found in the urine either then or at any other time; neither were any symptoms located in the bladder. This illness lasted about two months.

In May and June of 1888 he again had attacks of hematuria, and from September 17 to October 31, 1888, he has had nine attacks, passing as much as six or eight ounces of blood, he thinks, in some of the attacks. He has never passed any calculus. In the interval between the attacks the urine was clear. No cause can be assigned for the attacks; not uncommonly they have come on while he was lying in bed. He states that the right kidney is now the seat of marked aching pain.

Condition before operation.—Bladder was sounded, but no stone was found. Prostate not much, if at all enlarged. Renal dulness on the two sides equal and normal. Right kidney tender. Two species of urine were furnished, one with a large bloody sediment, but without clots, and the other clear and acid, sp. gr. 1022; very slight amount of albumin. Microscopical examination showed no crystalline elements, a few blood disks, granular matter, and a large number of bright fatty-like small globules. Dr. Watson, the patient's physician, had never found any albumin except just after the attacks of hematuria, nor has he ever seen any cast. It was decided to explore the right kidney, either for stone or possibly for cancer, and either to remove the stone or the kidney, as might seem best.

Operation, Nov. 3, 1888.—An oblique incision was made just to the right of the erector spinæ. The lower end of the kidney appeared normal. The finger detected a rather

sharp irregularity deep in the substance of the kidney. The moment it was pressed on we were convinced that it was a stone. A needle was then passed into the kidney, and the point of it grated with great distinctness against the supposed stone. The kidney was now seized with a volsella, and was loosened from the surrounding tissues in order to obtain freer access to it. This was followed by two results: First, very abundant, indeed very alarming hemorrhage, from large veins that were so concealed under the last rib that they were seized with great difficulty, even after the rib was well raised, and when seized they were so friable that the ligatures would not hold. The second result was to disclose the fact that while the small portion of the kidney first discovered was normal, the rest of it was irregular, nodular, and friable, and evidently the seat of a malignant growth. Accordingly, it was determined to remove the kidney, if possible. It was rapidly detached from its capsule by the finger, but it was so anchored internally at the hilum that it could not be brought to the surface, in spite of the fact that he got his entire hand into the cavity of the capsule.

Having proved the impossibility of removing the kidney by the loin, he had debated the question of attempting it by an anterior incision, but as the difficulty of removal was not the size of the kidney, but the adhesions at the hilum, he concluded not to attempt an operation by this route, as he felt convinced that it would result in the patient dying upon the table. The hemorrhage had been exceedingly profuse, not from any one particular vessel, or from rupture of the vessels of the hilum, but from every point in the kidney and in the capsule the moment they were separated. This hemorrhage was checked by thoroughly packing the wound with sublimate gauze. The patient was put to bed. He became conscious and recognized his family, but died from exhaustion three and a half hours after the operation.

Autopsy.—In order to determine whether the kidney could have been removed more readily by the anterior incision, Dr. Keen made this attempt as the first step in the autopsy. An incision was made in the right linea semilunaris, which gave no more room for the removal of the kidney anteriorly than posteriorly. The ribs projected so far downward that, in order to reach the kidney, it was necessary to insert his entire hand up to the wrist. The kidney lay far up under cover of the ribs, and was as inaccessible from the front as from the back. It was so thoroughly anchored in its position that to loosen it from its bed required force that would have been wholly unjustifiable during an operation, and would have resulted in rupture of the vessels and in immediately fatal hemorrhage. It would not have been possible to reach and tie the vessels in such an inaccessible position. When removed, the kidney was found to be enlarged, nodular, and distinctly cancerous. The left kidney and other abdominal viscera were normal.

On section of the kidney there were discovered some calcareous vessels and one or two points of calcification of the other tissues. Microscopical examination showed that it was an intermediate form between scirrhus and encephaloid, with a decided preponderance in the greater part of the organ of the latter form of the disease.

The diagnosis. This lay most likely between stone in the kidney and cancer of the kidney. Although it seemed unlikely that stone should exist without producing pyelitis, and, therefore, showing some pus in the urine, yet

Dr. Keen had known of more than one case of both stone in the bladder and in the kidney in which the urine contained no pus. The repeated hematuria looked very much toward malignant disease, but the kidney was so under shelter of the ribs that it was impossible to detect any tumor, and the dulness was not markedly increased. The enlargement of the kidney was chiefly toward the hilum, and so the dulness posteriorly was little greater than normal. Mr. Henry Morris states that of 30 cases of cancer of the kidney, found in 2610 autopsies, 25 were secondary, and only 5 were primary. The present specimen is undoubtedly a primary malignant tumor, and is, therefore, a rare form of disease.

In the surgical aspect of the case there are two points of interest: First, the needle test for stone. When the kidney was exposed to view, the only healthy portion of it remaining was first seen. Deep under this an irregular, hard mass could be felt, which might easily be a stone. Puncture by the needle convinced us that it was such. Examination of the kidney after death showed us that no stone existed, but that what was felt by the point of the needle was either a calcareous vessel or a calcareous degenerative mass against which the point of the needle grated. It gave precisely the same sensation as a stone would have done. This possible error seems very unusual, and he had never seen it noticed.

Secondly, the advantages of the lumbar or of the abdominal route for removal. As the operation was undertaken primarily for exploration, and no tumor in any sense was discovered, he was clearly of opinion that the lumbar route was the proper one to select. The attempt made at the autopsy shows that the kidney could not have been removed any more readily by the abdominal than by the lumbar incision. The peculiar situation of the mass in question, and the low position of the ribs, resulted in the curious fact that whilst the space between the last rib and the crest of the ilium was only two fingers in breadth, yet the oblique incision here of four inches was long enough for removal, and it could have been still further prolonged anteriorly, if necessary; whereas, the vertical incision from the rib to Poupart's ligament was absolutely limited to four inches, and the kidney was certainly no more accessible by this route than by the other. The removal of the kidney was practically impossible by either method. The inflammatory attachments—especially around the hilum and the great vessels of the kidney—required an amount of force that would have been unjustifiable during life.

DR. DEFOREST WILLARD thought that, as a rule, the anterior median incision offers better opportunities for diagnosis, for examination of the other kidney, and for safe removal. In cases where a stone is suspected, or the presence of pus is probable, the lumbar incision is certainly proper; the operation may be simply a nephrotomy and not a nephrectomy, and then we have better drainage. In simple purulent kidney, it is better to secure drainage and not remove the kidney. The results, so far as life is concerned, are better by this method. In tuberculous kidneys, the results of nephrectomy have been more satisfactory. In Bardenhauer's statistics,¹ numbering some thirty-five nephrectomies, where twenty-five were for purulent kidney of various forms, the mortality

¹ Berlin klin. Woch., October 15, 1888. THE MEDICAL NEWS, December 1, 1888.

was not much higher than in ordinary nephrectomy. Out of thirty-five cases he lost ten.

The choice of the incision will depend largely upon the condition of the case, and upon the diagnosis. Many nephrectomies have been performed after the abdominal incision has been made for other purposes, as when the ureter has been cut in laparotomy. He did not know why it was any better, but Schmidt recommended that in such cases the kidney be removed by the lumbar incision. It seemed to him that this would decidedly delay the operation, and would be more likely to cause contamination of the abdominal cavity. He did not think that the results were much more serious in the anterior operation than they were in the posterior.

In regard to the sutures to be employed in *nephrorrhaphy*, he thought that the failures had resulted from the use of catgut. We must employ a permanent suture, which will hold for a long time, and anchor the kidney until it is thoroughly fixed in its position. The question of decubitus is of importance. A month is the shortest possible time in which we can expect any fixation. The use of pads is very unsatisfactory. We are obliged to apply the pad upon the abdominal walls to an organ that lies deep in the loins with the intestines in front of it. We cannot hope to hold it in position by any such means. Therefore the dorsal decubitus should be maintained for a long time.

DR. M. PRICE thought the anterior incision the better, even if after making the diagnosis you close the abdominal incision and remove the kidney by the lumbar method. An incision one and a half inches in length affords ample room for the examination of all the organs. He had twice examined both kidneys through such an opening. In one case of supposed gall-stones, he found the viscera attached to the abdominal wall. After separating them, he examined the kidneys without difficulty, and found them in good condition. In the case where he removed the kidney, he had no difficulty in feeling both kidneys through a small abdominal incision. He thought that if Dr. Keen had made the median abdominal incision, and found the kidneys so seriously diseased, he probably would not have made as great an effort as he did through the lumbar incision.

His impression was that the anterior route was the best for drainage, which, in these cases, is of paramount importance. So much tearing is done in releasing the kidney that drainage for twenty-four or forty-eight hours is necessary, or at least can do no harm. He did not believe that any drain answered its purpose so well as one that can be cleaned by the attendant or nurse. The best method to stop oozing of blood is to keep it cleaned away. Keep no blood in contact with the bleeding vessels. He believed that morphia had a tendency to cause suppression of urine. It also lowered the vitality and assisted in killing the patient. He should not think of using morphia in abdominal cases unless the patients were dying. Where morphia had been used in surgery, and especially in abdominal surgery, he had had cause to regret it.

DR. JOHN B. DEEVER had operated both by the abdominal and lumbar method. He considered that the anterior incision was preferable in cases of solid growths, particularly where they have reached any size. We run greater risks in attempting to break up the adhesions to the capsule by the lumbar incision, where we cannot see

what we are doing, than we do through the anterior incision. The proper course is to go through the linea semilunaris. This brings us nearer to the organ, and gives a better opportunity to work to the outer side of the colon, which is important, as the bloodvessels are in relation to the internal layer of the meso-colon, and not with the external layer. In cases of liquid accumulations in the kidney, he did not think that we operated with quite as much facility anteriorly as posteriorly. The lumbar incision affords better opportunities for drainage under these circumstances, but drainage can be satisfactorily accomplished by the anterior method by the glass drainage tube. The abdominal incision affords us better opportunities for the examination of neighboring organs. For purposes of diagnosis it is more satisfactory than the operation through the loins.

There is no doubt that in *nephrorrhaphy* the proper form of suture is antiseptic silk. He knew of one failure resulting from the use of catgut. It is probably impossible to pass sutures through the capsule of the kidney without also involving the substance of the organ, yet he thought that if we could avoid wounding the kidney it would be better. He had no doubt that the appearance of albumin in Dr. Keen's case after the operation was due to the sutures.

The question of the removal of solid growths of the kidney is largely influenced by the age of the patient. It is almost useless to operate for carcinoma of the kidney in early life or in late life, so that the middle period offers the best chances for a favorable result in these cases.

DR. THOMAS R. NIELSON said that the statistics of the removal of the kidneys for malignant disease are so unfavorable that any operation seems to be almost hopeless; nevertheless he did not think that, in individual cases, this should be a contra-indication if the operation seems to be justifiable on other grounds. In children, the statistics of Dr. Gross, in 1885, were, that in thirteen operations only four recovered from the operation, and these subsequently died from return of the disease elsewhere.

DR. JAMES TYSON said in reference to the etiology of floating kidneys, that he had been compelled to conclude that in the majority of instances it is congenital. He had seen most typical instances of floating kidney in men, and had seen it more frequently in women who have never borne children than in those who have had children. Even in those cases where it is supposed to have been caused by accident, that in all probability the condition has been congenital, and, if anything, only exaggerated by the fall. In regard to the treatment of floating kidney by the use of pads, etc., he had never seen any advantage result from such devices.

DR. J. M. BALDY said that there is great importance in drainage in kidney operations. A tube that can be kept clean, is the drainage tube *par excellence*—the glass tube is such.

In one case of malignant disease, in which he witnessed the removal of the kidney by the abdominal incision, the hemorrhage not being entirely controlled, the peritoneal cavity was entirely closed off from the bed of the kidney by stitching the cut edges of the peritoneum together; prior to this, however, a counter-opening was made through the muscles of the back, and thus good drainage secured. The patient made a good recovery.

DR. JOHN B. ROBERTS referred to a case of cure of movable kidney without operation. A boy, seven years of age, was referred to him four years ago by Dr. M. O'Hara. At irregular intervals the patient was seized with severe pain in the left side of the abdomen, and with this there was the appearance of a tumor in the hypochondrium and total suppression of urine. Various theories had been held to explain the condition. The only conclusion that he could reach, although this was not concurred in by Dr. O'Hara, was that the boy had a floating kidney, and that at the times of the paroxysms the ureters became twisted, causing the suppression of urine and the intense pain. The boy never had any pain or trouble with the urine except when the tumor was felt. At his suggestion Dr. O'Hara had made a pad which pressed against the left hypochondrium. This was worn for a short time. The boy then passed from his notice, but he learned a few years ago that he had perfectly recovered. There is, of course, in this case an element of uncertainty in regard to the diagnosis. When he looked into the literature of this subject, he was surprised to find how much stress was laid upon the possibility of the ureters becoming twisted, and the flow of urine being interfered with.

In connection with the question of the lumbar incision he referred to an accident which he had in an attempt to explore the kidney for a supposed renal calculus. After making the incision down to the kidney, he determined to enlarge it a little. The diaphragm hung down in a fold and was readily seen. He made the incision a little longer in the upward direction, and made a minute perforation in the diaphragm where it is attached to the spinal column. There was a loud whistle as the air rushed into the pleura. He put in a suture and closed the opening, and no harm was done; but for a few days the patient insisted that the bandage was too tight, because he could not breathe freely. The pain which the patient had felt disappeared after the operation, although no stone was found. He went home before the wound had closed, and a number of months later died of some obscure disease.

It is important to recollect, when operating close to the vertebral column, that the posterior attachment of the diaphragm near the middle line extends further down than might be realized from watching its loose muscular curtain exposed in the wound.

DR. W. W. KEEN thought that there was no doubt that in the case of a large tumor of the kidney, particularly a large solid tumor the size of which cannot be diminished by tapping, the position taken by several of the speakers is correct—that the anterior incision is the proper one. Where there is a small growth, or a stone in the kidney, or the operation is an exploratory one, he could not think that the anterior incision is the best. Statistics certainly show that the lumbar incision is attended with much less risks than the anterior. Where there is no reason to suppose that an unusual amount of room will be required, he thought the lumbar incision is the proper one to employ.

It has been also suggested that possibly the anterior incision in the present case of nephrectomy would have given so much information that he should have decided not to operate. He did not think that he should have reached any such conclusion, for it was not the character of the growth, but the adhesions at the hilum that inter-

fered with the removal of the organ, and these could not have been determined until the operation was in progress. He thought that it would have been as impossible to remove the kidney by the one method as by the other. He had the pleasure of seeing Dr. Steinbach's case just prior to the beginning of the operation, and he certainly was convinced that it was a case of enlarged gall-bladder with gall-stones. The slightest movement caused a grating of one stone upon the other. The tumor was in the position of the gall-bladder. He never before saw a kidney so displaced. The long axis, instead of being vertical, lay in an antero-posterior position, and the upper end of the kidney occupied precisely the position of the gall-bladder. The mistake in diagnosis, under these circumstances, was very natural.

NEW YORK ACADEMY OF MEDICINE.

SECTION ON PUBLIC HEALTH.

ALEXANDER HADDEN, M.D., IN THE CHAIR.

DR. C. G. CURRIER read a paper on

THE EFFICIENCY OF FILTERS AND OTHER MEANS EMPLOYED TO PURIFY DRINKING-WATER, BACTERIOLOGICALLY CONSIDERED.

(See page 425.)

DR. T. MITCHELL PRUDDEN said that a good deal of objection had been made to the work of bacteriologists, and largely on the ground that it had been found very difficult by them to detect the typhoid bacillus in drinking-water. It was purely a technical matter, and the trouble was the difficulty of distinguishing the typhoid bacillus from other bacilli which closely resembled it. This applied principally to large bodies of water, since in small bodies of water the typhoid bacillus could be much more readily recognized. Dr. Prudden then spoke of the serious epidemic of typhoid fever which occurred in Providence, R. I., in the early part of the winter, and to which reference had been made in the paper. It seemed that last autumn typhoid fever was prevalent in a village of French Canadians situated on the river from which the water-supply was derived, and about half a mile above the city. The sewage of the village did not pass directly into the river, but the inhabitants emptied the dejections from typhoid fever patients into privy vaults and also upon the banks of the river. In November there was an unusually severe rain-storm, which washed a large amount of material from the banks into the stream, and early in December the death-rate in Providence rose to 127. Notwithstanding the fact that the flow of water amounted to over seven million gallons per hour he was able to detect a few typhoid bacilli, together with a large number of ordinary water bacilli, in the charcoal filter referred to by Dr. Currier. It consisted of an outer and inner metallic ball, and was so arranged that the water could pass around the charcoal, while there was an orifice half an inch in diameter from which the water escaped.

DR. JOHN C. PETERS said that it seemed that to-day the water of both Berlin and London was more free from contaminations than that of New York. The filter-beds employed in purifying the water-supply of London were mostly of sand, the efficacy of which Dr. Currier had spoken of in the paper. Dr. Peters said that during

the past four years he had paid considerable attention to the condition of the Croton water-supply, and he had been glad to learn that the State Board of Health had undertaken the work of keeping it as pure as possible in the future. At present the amount of contamination that got into the water was very great; yet it was a fact that during the past season there had been a smaller number of cases of typhoid fever reported in the city than for a long time before.

DR. R. C. M. PAGE said that for a number of years past, up to last summer, he had used in his house a globe filter about three inches in diameter, containing clean sand. At first tin was the material used in its construction, but afterward copper was substituted for it, as the tin soon rusted out. Last summer, in Paris, he met with the filter spoken of by Dr. Currier in the paper, which he now exhibited, and which he said was the only filter which had really ever proved satisfactory to him. It somewhat resembled the Pasteur filter, and consisted of a large disk of porcelain backed by carbon. It allowed the water to pass much more quickly than the Pasteur filter, and two quarts an hour could be obtained from it. He thought that the water from such a filter was better than distilled water, as a rule, since one could never feel sure that the latter, as ordinarily supplied, was freshly distilled.

DR. J. LEWIS SMITH said that from recent experiments referred to in a lecture by Jules Simon, of Paris, it would appear that a filter which would arrest the passage of the Klebs-Löffler bacillus, would permit the passage of ptomaines resulting from microbic action. This seemed to him a very interesting point.

DR. AGRAMONTE inquired if Dr. Currier had ever made an examination of the Apollinaris water which was so largely used at the present time, and so highly recommended for its purity.

DR. CURRIER stated that he had not examined this particular water.

DR. JOHN C. PETERS said that he had reason to believe, on good authority, that not one-half of the Apollinaris water put upon the market was derived from the Apollinaris Spring. A considerable proportion of it, at least, was artificially prepared at Antwerp, where this could be done very cheaply.

DR. CURRIER inquired if any of those present could inform him what was the effect of distilled water upon children and youth, particularly as regards the lack of salts, of which the water was deprived by the process of distillation.

DR. H. JACOBI said that the salts in drinking-water that could be expected to be beneficial to the system were the chlorides and phosphates. The Croton water, when pure, was regarded as a very excellent water, and yet it came very near distilled water in regard to the absence of salts. Some of the hot spring waters in Europe also contained very few salts. He believed, however, that we all suffered more from the presence of salts in drinking-water than from the lack of them. When we did drink hard water, the salts merely passed through the system without being utilized. It was, unquestionably, a fact that children, as well as older persons, took into their systems, from milk and other liquids, and from both farinaceous and animal foods, more salts than they could possibly utilize. Hence he did not believe that distilled water was detrimental to health on account of its lack of salts.

CORRESPONDENCE.

THE VALUE OF RHUS TOXICODENDRON IN RHEUMATIC AFFECTIONS.

To the Editor of THE MEDICAL NEWS,

SIR: The discussion which appeared in the columns of your journal of April 6th, relating to the treatment of sciatica, has attracted my attention, and having been over the ground traversed by most of the participants, I take the liberty of suggesting a comparatively new remedy for the relief of such cases. The hypodermatic use of osmic acid and the glass rod have not come within my personal observation, nor have I had experience with the excision of the affected nerve, nor with stretching, although I have seen the operation. Few persons, however, are willing to submit to this heroic method, since no guarantee is afforded that the results will be successful. The application of the instrument devised and used so successfully by Charles Bauschmidt has frequently been followed by the very best results, but I have not been able to determine just the class of cases in which it would answer. That it is a successful method of applying counter-irritation cannot be denied, and where patients do not object, there are many instances in which it may be used with great advantage, the indications for its use not being confined to rheumatic affections. Some years ago I recall the circumstances attending two cases of sciatica, both of which were operated upon in this way the same day. In one case there was no benefit whatever, while the other was immediately and permanently cured. The first case was that of an elderly gentleman who had been under treatment for some time and whose history will be related as offering us some interesting items in connection with the care of such cases. The second case just mentioned was that of a lady aged about thirty-five, who had been suffering for some months, and was somewhat anæmic, and in addition there was prescribed for her a suitable tonic.

The remedy which I would put forward as a material addition to our therapeutics in the treatment of many of the more obscure manifestations of rheumatism is *rhus toxicodendron*, a remedy which has attained some local celebrity in different sections of this country, and one too, which has been used by a number of practitioners abroad. The drug is referred to by Dr. Phillips in his excellent treatise, and from this we learn that it was first brought to the notice of the profession in 1798 by Dufresnoy, a physician of Valenciennes. Later, some interesting observations were made upon it by Alderson of England, and in 1836 it was included in the London Pharmacopœia. Dr. Phillips suggests that *rhus* is valuable in various subacute and chronic rheumatic affections of fibrous tissues, and that the synovial membranes seem to be less amenable than the fibrous structures, such as tendons, ligaments, and fasciæ. He also recommends its use both locally as a liniment, and internally in the later stages of rheumatism, "when the patient suffers still from wearing stiffness, and aching of subacute character in the joints." It is also recommended in the case of incontinence of urine, and is referred to by Drs. Whitla and Brunton in their respective works.

In the vicinity of Chester, a few miles south of this city, it is said to grow in luxuriant abundance, and in

that section its value has attained great, and judging from reports which have been made from time to time, warranted popularity. The poison sumach is also referred to in the latest edition of the U. S. Dispensatory, but not in very complimentary terms, the authors intimating that it is an extremely active poison and probably dangerous to be used as a remedy. Some reports have appeared in current medical literature, but as none of them, so far as I know, covers the ground upon which I have based my treatment, it may be worth while to offer several typical cases by way of illustration of the different directions in which its application may be anticipated, for the relief of diseased conditions. It may be mentioned here that my observations extend over a period of nearly three years, and that my first published communication on this subject appeared in December, 1887.

Case I.—My first case mentioned above was a gentleman, aged about sixty, who had been under treatment two years previously for the same affection, and was relieved by the deep injection of chloroform, together with the occasional application of the faradic current. At the time when he first came under observation he was suffering from cardiac derangement which culminated, from time to time, in attacks of unconsciousness, attended with great physical depression; the real cause probably being due to fatty degeneration with its attending circumstances. Appropriate treatment, however, overcame this disposition, and a year later he complained of sciatica. Although scarcely able to walk, he came to the office every few days, and shortly was able to walk several miles daily without any embarrassment, and the pain in the hip was comparatively easy. The following spring the pain again appeared, if possible, worse than before, and thinking I had exhausted the resources of the pharmacopœia, he consulted an irregular, who faithfully sand-papred him, and afterward applied some stimulating embrocation, which nearly drove him wild; in addition to this, he volunteered the information that his hip was out of joint, and that, although he could put it in place again, the bone refused to stay in position. This style of medication was continued for some weeks, and the patient then consulted an electrical physician, who guaranteed to effect a cure within a reasonable time, but to make the matter more binding, he took a retainer of twenty-five dollars in advance. Electrical treatment was continued for about two months, but without that relief which the patient had expected, and the expense proving a serious objection, it was discontinued.

This patient again consulted me in the September following, some time after the other methods of treatment had been discontinued, and was placed upon the salicylate of sodium, together with strychnine sulphate gr. $\frac{1}{10}$ after meals, and after a time it was deemed advisable to resume counter-irritation, but the patient strongly objected to the injection of chloroform, although he was willing enough to have a blister. Instead, however, Baunscheidtismus was adopted, but without any apparent benefit. Later on, a thapsia plaster was applied, and seemed to serve the purpose for the time being, but the pain was soon as severe as before.

During the interval of relief afforded by the thapsia plaster treatment had been discontinued, so that at the expiration of two weeks, it may be said, this patient was wholly free from the influence of any medication what-

ever, and it was at this period that rhus was ordered, one drop of a solution of the tincture, three or four times daily, and in less than a week the sciatica had yielded. The best part of the story is, that now more than two years there has been no reappearance, and the extra "lift" on the boot-heel, which had been added to make up for the dislocation, and which was discarded at my suggestion, has never been reapplied.

Case II.—About five years ago, a farmer, aged fifty, consulted me on account of lameness in one knee, from which he had suffered for fifteen years, and was relieved by the local application of an ointment of salicylic acid and lanolin. Two years later, he applied for the relief of lameness of the shoulder, which had developed gradually, and resembled the lameness of the knee which had crippled him for so many years. Here the rhus was exhibited internally in small doses and applied externally as a liniment, and in the course of a few days, the pain and stiffness had disappeared, and the patient has since been free from any of the symptoms of rheumatism.

Case III.—A farmer, aged about sixty-five, was attacked last summer with sciatica of very painful nature, which seriously interfered with his work and his comfort, disturbing his rest at night. In this case there was not a distinct history of rheumatism, but there was a history of general debility, and while we cannot charge rheumatism to that alone, there frequently seems to be a direct association of the two. It is sufficient to add that the patient recovered from the use of rhus alone; no medication preceded it, and none has followed.

Case IV.—Whether varicose veins may be regarded as one of the manifestations of rheumatism, is a question which will require further elucidation, but the fact that the remedies which are useful for the relief of one affection are often of benefit in the treatment of the other, would lead us to suspect some occult relationship. Mr. B., aged thirty-five, unfortunately was subject to both, and when one did not trouble him the other did, so that life became a burden to him. The rhus was taken in moderate doses for a time in the hope of relieving the rheumatism, and his surprise was all the greater when he found that the varicose condition of the veins of the legs was greatly benefited as well as the rheumatism. This patient eventually recovered, notwithstanding the assertion of numerous physicians that nothing could be accomplished for him without an operation, and rhus was the only remedy.

Case V.—The following case is fresh in my mind, the patient, a married woman, aged thirty, having come under observation for the first time about two months ago. At each menstrual period, and at other times when she was compelled to work hard, the enlarged veins of the legs were almost unbearable, and it was almost impossible for her to rest at night. Small doses of rhus removed all pain in the course of a few days, and since that time she has been taking the drops for the most of the time; there has been no pain of any consequence, either during the menstrual molimen, or when she has been working hard in looking after a large family. The puffiness of the vessels has not disappeared, but their appearance is most promising, and it seems as though greater rapidity could be secured by the application of a suitable bandage, provided the patient appreciated the necessity of keeping it in place.

Case VI.—It not infrequently happens that a physician is called upon to relieve attacks of "cramps in the legs," occurring at night, the patient saying that suddenly he is awakened from a sound sleep by cramps, that it is impossible for him to remain in bed, and he jumps out on the floor. Most persons subject to attacks of the kind have provided themselves with a strong cord for such emergencies, and at the moment of attack they coil it around the leg once or twice, then, holding an end in each hand, they give it a short, strong jerk, which is said to relieve the cramp quicker than anything else.

Mr. G. had suffered from this trouble for several years, and came to me for relief. He had a cord, and he also told me that a doctor once gave him medicine which relieved him for a long time. Knowing the value of rhus in relieving the pain which affects varicose veins, I concluded that it might be of some value here, and was not disappointed. Treatment was a success, and has been adopted with like results in other cases of the same character.

Case VII.—Mr. B., aged thirty, is a successful and apparently happy farmer, who has been engaged in his present work for the past two years. His health is impaired by reason of rheumatic symptoms, and for the last five years, he says, the same conditions have existed to a greater or less extent. Previous to the time of adopting farming as a business, he had been engaged as a private coachman, and received good wages without having to work too hard or unnecessarily expose himself to inclement weather. He complains of fugitive pains about the larger joints, and says the water causes a very unpleasant sensation when passed from the bladder; there seems to be an unusual sensation of heat, and yet he does not pass a very large quantity, but rather less, he thinks, than in former years. An examination shows that it is normal as to gravity, but the acidity is particularly marked, and when a drop is placed under the microscope the field is dotted with crystals of different varieties, probably uric acid in combination.

This patient had been a faithful patron of the doctors, first the regular, then the irregular, and his last exploit was the submission of his case to a druggist. The last was the worst of all, and he finally rebelled, and concluded to try the whole thing over again. A weak solution of the tincture of rhus was ordered, five drops to be taken three times daily in a little water. When he reached home and compared the small vial containing his medicine with the formidable bottles he had been in the habit of getting, he was inclined to regard the matter in the light of a good joke, but concluded to undertake it, thinking my purpose was to learn if he would take the medicine faithfully. In a few days he reported that he was able to report progress, and the medicine was continued for another week, except that the dose was reduced to twelve drops daily. Now, at the expiration of ten days from the time treatment was first instituted, this patient reports that the trouble with the water has ceased, that the rheumatic symptoms have subsided, and that he feels better than at any time for five years.

In concluding this somewhat lengthy communication, which could be extended indefinitely, as I use the remedy nearly every day, let me suggest to the gentlemen engaged in the discussion to which I have referred, that it would be interesting to have the benefit of their

observations regarding the value of this remedy which I have inadvertently called new, which holds out such great promise as a therapeutic agent.

JOHN AULDE, M.D.

4719 FRANKFORD AVE., PHILADELPHIA, APRIL 8, 1889.

NEWS ITEMS.

The Medical Society of the County of New York Sustained.—

Judgment has been entered on a final order of Judge Ingraham, of the Supreme Court, sustaining the Medical Society of the County of New York in its expulsion of Dr. James O'Reilly for practices in violation of the Society's code of ethics.

OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF OFFICERS SERVING IN THE MEDICAL DEPARTMENT, U. S. ARMY, FROM APRIL 9 TO APRIL 15, 1889.

BYRNE, CHARLES C., *Surgeon* (U. S. Army).—Promoted Surgeon, with the rank of Lieutenant-Colonel, to rank from March 29, 1889.

MUNN, CURTIS E., *Assistant Surgeon*.—Promoted to Surgeon, with the rank of Major, to rank from March 29, 1889.

By direction of the President, PAUL R. BROWN, *Captain and Assistant Surgeon*, will report in person to Brigadier-General John R. Brooke, President of the Army Retiring Board, at Omaha, Neb., for examination by the Board.—Par. 9, S. O. 80, A. G. O., Washington, April 6, 1889.

By direction of the President, the State of Wisconsin is transferred from the Department of the East to the Department of Dakota.—G. O. 36, A. G. O., Washington, April 6, 1889.

OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF THE MEDICAL CORPS OF THE U. S. NAVY, FOR THE WEEK ENDING APRIL 13, 1889.

HEFFENER, A. C., *Passed Assistant Surgeon*.—Found unfit for duty at present, by the Retiring Board, but not permanently incapacitated for active service, and granted one year's leave of absence for medical treatment.

DU BOSE, W. R., *Passed Assistant Surgeon*.—Detached from the U. S. S. "Constellation," and ordered to the Practice-ship "Jamestown."

LOWNDES, C. H. T.—Commissioned an Assistant Surgeon in the U. S. Navy, March 13, 1889.

OFFICIAL LIST OF CHANGES OF STATIONS AND DUTIES OF MEDICAL OFFICERS OF THE U. S. MARINE-HOSPITAL SERVICE, FOR THE TWO WEEKS ENDING APRIL 13, 1889.

GODFREY, JOHN, *Surgeon*.—To proceed to Poughkeepsie, N. Y., on special duty, April 10, 1889.

MEAD, F. W., *Passed Assistant Surgeon*.—To report in person to the Supervising Surgeon-General, April 3, 1889. Detailed as Acting Chief Clerk Marine-Hospital Bureau and Attending Surgeon port of Georgetown, D. C., April 10, 1889.

WHEELER, W. A., *Passed Assistant Surgeon*.—Relieved from duty at Buffalo, N. Y., to assume charge of the Service at Norfolk, Va., April 3, 1889.

DEVAN, S. C., *Passed Assistant Surgeon*.—Relieved from duty as Acting Chief Clerk Marine-Hospital Bureau and Attending Surgeon, to assume charge of the Service at Buffalo, N. Y., April 3 and 12, 1889.

PETTUS, W. J., *Assistant Surgeon*.—Granted leave of absence for four days, April 6, 1889.

STONER, J. B., *Assistant Surgeon*.—To rejoin station (New York) as soon as practicable, April 11, 1889.

THE MEDICAL NEWS will be pleased to receive early intelligence of local events of general medical interest, or of matters which it is desirable to bring to the notice of the profession.

Local papers containing reports or news items should be marked. Letters, whether written for publication or private information, must be authenticated by the names and addresses of their writers—of course not necessarily for publication.

All communications relating to the editorial department of the NEWS should be addressed to No. 1004 Walnut Street, Philadelphia.